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HUXLEY.

Life and Letters of Thomas Henry Huxley, F.R.S. By Leonard Huxley. Vol. I., pp. viii + 503; Vol. II., pp. vi + 504. (London: Macmillan and Co., Ltd., 1900.) 30s. net.

THE real life of Huxley has still to be written. What is wanted is a critical study of the development of his striking personality and an estimate of the work of his life and the effect it has produced. I have nothing but praise for the two bulky volumes of the "Life and Letters," in which a filial duty has been accomplished with taste and judgment. But though they supply invaluable material they do not attempt to bring the facts of either career or performance to a clear focus.

Such a study in competent hands would be a fascinating undertaking. It would not merely give a picture of a very remarkable man, but would give also a chapter in the history of English science of supreme importance. I make no pretension to ability for the task myself, even if the columns of this Journal could afford the space. But I shall hazard the attempt to indicate the essential points which I should like to see more amply treated. I have gathered the material from a pretty close study of the "Life and Letters," and I have added the references of volume and page to quotations, which are not always easy to find, for any one who cares to verify them.

Nothing in tracing an eventful career is so attractive as speculation on the "might-have-been." It is probable, however, that within narrow limits "circumstance" counts for little beyond giving a dramatic touch to the story. But it played its part again and again in Huxley's life for what it was worth.

His family traces back to the north-west of England, where a certain fibrousness of character is commoner than in the south. His father was a master in Dr. Nicholson's school at Ealing, where Huxley was born in 1825. He describes himself as "a thread-paper of a boy" (ii. 35) with "a wild-cat element in me" (i. 5). For education in the ordinary sense:—

"I had two years of a pandemonium of a school (between 8 and 10), and after that neither help nor sympathy in any intellectual direction till I reached manhood" (ii. 145).

The school came to grief and Huxley's father moved to Coventry. Huxley was left to his own devices. What they were is almost incredible; but then he has told us that "*a priori* reasonings are mostly bosh" (ii. 212). At twelve he was sitting up in bed before dawn to read Hutton's "Geology" (i. 6). His great desire was to be a mechanical engineer; it ended in his devotion to "the mechanical engineering of living machines" (i. 7). His curiosity in this direction was nearly fatal; a *post mortem* he was taken to between thirteen and fourteen was followed by an illness which seems to have been the starting point of the ill-health which pursued him all through life. At fifteen he devoured Sir William Hamilton's "Logic." Twenty years later he says:—"From that time to this ontological speculation has been a folly with me" (i. 218). At seventeen he came under the influence of Carlyle. Nearly fifty years later he wrote:—

"There is nothing of permanent value (putting aside a few human affections), nothing that satisfies quiet reflection—except the sense of having worked according to one's capacity and light, to make things clear and get rid of cant and shams of all sorts. This was the lesson I learnt from Carlyle's books when I was a boy, and it has stuck by me all my life" (ii. 268).

At the same age he began his regular medical studies at Charing Cross Hospital with his brother, to whom Newman (afterwards Cardinal), who had been educated at the Ealing school (i. 19), gave a testimonial. He attended Lindley's lectures at the Chelsea Botanic Garden and won one of the medals of the Apothecaries Society. At the Medical School he studied under Wharton Jones, a physiologist who never seems to have attained the reputation he deserved. Perhaps he got mixed up with "the other fellow," who, Huxley thought, had "mistaken his vocation" (i. 94), an opinion in which, from personal experience, I can quite agree. Of Wharton Jones, Huxley says:—

"I do not know that I ever felt so much respect for a teacher before or since" (i. 21).

At twenty he went up for his First M.B. examination at the University of London, winning the gold medal for anatomy and physiology. Ransom, of Nottingham, won the Exhibition. Here circumstance came in.

"If Ransom had not overworked himself .... I should have obtained the Exhibition .... and should have forsaken science for practice" (ii. 133).

Would he?

Something had to be done to get a livelihood, and at the suggestion of a fellow student, now Sir Joseph Fayrer, he applied for an appointment in the Navy. Circumstance again, he came under Sir John Richardson, himself no mean naturalist, and through his influence was attached to the *Rattlesnake*. One of the oddest things about Huxley's career is the fact that almost every one he had to do with turned out sooner or later to be somebody notable. Through his Captain, Owen Stanley, "a thorough scientific enthusiast" (i. 25), he was introduced to Owen, Gray and Forbes, the first and last of whom had a good deal to say to his future career. The voyage of the *Rattlesnake* occupied four years. Huxley was twenty-five on his return. Few scientific men ever used their opportunities with keener sagacity. He spent no time in mere collecting. But, with an instinct which appears to me altogether extraordinary in one who was little more than a youth fresh from a medical school, he seized upon everything that was important and with regard to which new ground was to be broken; and, characteristically, he steadily kept their physiological interest to the front. The rest may be passed over rapidly; he had, in a scientific sense, his reward. His paper on the structure of the Medusæ had been published during his absence in the *Philosophical Transactions*. In this paper he laid down the fundamental character of the "ectoderm and endoderm." As Allman justly remarks, "this discovery stands at the very basis of a philosophic zoology" (i. 40). It is not too much to say that it is the foundation of modern zoological theory, and had Huxley never done anything

else he would still have retained a classical place in its history.

At twenty-six he was elected a Fellow of the Royal Society. At twenty-seven he not merely received the Royal medal, but was placed on the Council. Certainly, half a century ago, our venerable Society showed no want of alacrity in recognising rising merit. And if any one wants to suggest that it has become less active in that respect he may be reminded that it was equally prompt in the case of Hertz.

It is certainly a notable circumstance that three men who were contemporaries and ultimately close friends, Darwin, Hooker and Huxley, each began his scientific career on board one of Her Majesty's ships. The consequence in each case was momentous to science: Darwin gave us the "Origin," Hooker a rational theory of geographical distribution, Huxley a reformed zoology. The odd thing is that while the two former returned confirmed naturalists the latter came back as impenitent as ever, and never was of a better mind till quite the end of his life.

At the age, then, of twenty-seven Huxley had placed himself with absolutely no aid in the very front rank of English scientific men. "What makes," he says, "the bigwigs so marvellously zealous on my behalf I know not. I have sought none of them and flattered none of them" (i. 78). Yet he did not emerge from the struggle altogether unscathed. Writing to Kingsley some eight years later he for once allows the cry of the wounded heart to escape him :—

"Kicked into the world, a boy without guide or training, or with worse than none, I confess to my shame that few men have drank deeper of all kinds of sin than I have" (i. 220).

Frankly, I do not believe a word of it. My experience of life does not lead me to think that any one who begins with a rank crop of weeds is very likely to ever reap a more substantial harvest. The plain fact is that the mood of confession is a perilous one, and from St. Augustine onwards most men who have yielded to it have found a sort of painful satisfaction in painting their past in the blackest colours. But I am entirely unable to find any point in Huxley's youth at which I can fix that outburst of the natural man. In a rather serious conversation I once had with him he spoke of a period in his life when he *might* have taken to evil courses; but he did not give me the smallest reason to suppose that in the retrospect he saw more than the existence of a possible *crevasse* in his path into which he might have fallen.

If Huxley's scientific reputation was established, his material position was still unassured. "Nothing," he says, "but what is absolutely practical will go down in England. A man of science may earn great distinction, but not bread" (i. 66). The struggle, however, in his case, if sharp, was less prolonged than it has been in the case of many other men. Owen got him his first temporary appointment (i. 95). Edward Forbes, "a regular brick"—an opinion I never heard any one gainsay—backed him "through thick and thin" (i. 107). He refrained, therefore, happily, from abandoning "all his special pursuits and take up chemistry, for practical purposes" (i. 86). He had tried to get "crystallised carbon" at fifteen (i. 10)!

Huxley was now thirty, and at last happily married. He might have succeeded Forbes in the Edinburgh chair, but "preferred to live in London on a bare sufficiency" (i. 120). He settled down at the School of Mines; his ship had come into port; what was the cargo he brought with him? As a boy he conceived a profound distrust of metaphysical speculation; at fifteen he writes in his note-book, hammered out from "Novalis," "Philosophy bakes no bread" (i. 9); that he stuck to to the end. From Carlyle he learnt his empiricism, a determination to see things as they are. From Wharton Jones he acquired an exact method, and from the *Rattlesnake* voyage confidence in his own powers of observation and courage to criticise the word of others. And here I must interpose the remark that it is difficult to estimate the services which biological science in this country owes to our medical schools. Up to the present time without them it would possibly not have existed amongst us at all. Huxley later on was more willing than I am to kick away the ladder :—

"Our side has been too apt to look upon medical schools as feeders for science. They have been so, but to their detriment as medical schools. And now that so many opportunities for purely scientific training are afforded, there is no reason that they should remain so" (ii. 310).

For my own part, owing much to medical training, I entirely dissent. The foundation of medical studies on a scientific basis, far from being detrimental, has in my opinion been of incalculable benefit to them. If Huxley really contemplated a division between medicine and science it was the worst cause he ever advocated.

Huxley's official duties, much against the grain, brought him face to face with palæontological problems. This not merely led to some of his most brilliant work, but put a weapon in his hand which he used afterwards with irresistible effect.

Half a century ago Owen was the dominant, and I think it must be admitted an evil, influence in the English biological world. He was saturated with the "naturphilosophie" and the teaching of Oken. Huxley was bound to come into collision with this. The Croonian Lecture in 1858, "On the theory of the vertebrate skull," demolished Oken's theory, and with it "fell the superstructure raised by its chief supporter, Owen, 'archetype' and all" (i. 141). Owen had already felt that his throne was tottering and, having borrowed the lecture-room in Jermyn Street for a course of lectures, boldly assumed, without the smallest warrant, the title of "Professor of Palæontology at the School of Mines" (i. 142). For this and many subsequent proceedings of a like nature the only plausible explanation that I can see is lunacy.

Here again Huxley laid one of the foundation stones of modern biological science. In his paper on the Medusæ he supplied the key which has unlocked the secrets of embryology; his Croonian lecture, followed by the work of Gegenbaur, has placed vertebrate morphology on a scientific basis.

This was his first conflict with scientific idealism, but

it was a mere affair of outposts compared with the campaign that was to follow. He tells us :—

"I was not brought into serious contact with the 'species' question until after 1850. At that time I had long done with the Pentateuchal cosmogony . . . . from which it had cost me many a struggle to get free" (i. 167).

Later on he calls "the hypothesis of special creation . . . . a mere specious mark for our ignorance" (ii. 302). What was to be put in its place? Herbert Spencer, whose acquaintance he made in 1852, was unable to convert him to evolution (i. 168). He could not bring himself to acceptance of the theory—owing, no doubt, to his rooted dislike to *a priori* reasoning—without a mechanical conception of its mode of operation. Like Darwin, he derived no comfort from either Lamarck or the "Vestiges" (i. 168). For the former, nevertheless, he always entertained the most profound respect, and thought he would run Darwin "hard both in genius and fertility" (ii. 39). His review of the latter was the only one he ever had "qualms of conscience about on the grounds of needless savagery" (i. 168).

His attitude to evolution continued to remain altogether sceptical and stand-off. In his first interview with Darwin, which seems to have been about 1852, he expressed his belief "in the sharpness of the lines of demarcation between natural groups," and was received with a "humourous smile" (i. 169). Hooker, on the other hand, he thought "*capable de tout* in the way of advocating evolution" (i. 170); but then Hooker was in the secret.

Before continuing the story I think it will be helpful to state in simple terms the problem that Darwin attempted to solve, and to which he got his first clue in the Galapagos. Take a number of organisms at random and proceed to sort them according to their resemblances. When this has been done it will be found that they have fallen into groups larger or smaller, as the case may be. The members of the groups will closely agree in all essential particulars; they are *individuals*. Yet no two are exactly alike; this is *variation*. Yet within the group there will be nothing to oppose the view that each may pass into the other; the variation is *continuous*. This will not be the case in comparing groups themselves; the variation is more marked and *discontinuous*. The discontinuity can be expressed in technical terms, and these give us an abstract definition of the *species* or the distinctive marks common to the individuals forming the group. Treating species in the same way we arrive at a series of discontinuous groups of a higher order; these are *genera*. Continuing the process we obtain *families*. Proceeding onwards in the scale we find ourselves face to face with two, perhaps the most difficult of all to define—the Vegetable and Animal kingdoms.

Now Darwin, of course, saw with every one else that if the mode of origin of groups of the first order could be explained, all the rest followed. What was wanted was the discovery of some intelligible agency which could effect the passage of one organic form to another. As Huxley put it :—

"That which we were looking for, and could not find, was a hypothesis respecting the origin of known organic forms which assumed the operation of no causes but such as could be proved to be actually at work" (i. 170).

Darwin assumed continuous variation as an empirical fact and "natural selection" as the agency which had directed the course of organic evolution. This was a generalised form of the "artificial selection" which the cultivator and the breeder use every day in moulding organic nature pretty much as they will. As Huxley says :—

"My reflection when I first made myself master of the central idea of the "Origin" was, 'How extremely stupid not to have thought of that'" (i. 170).

Huxley's attitude to Darwinism deserves careful study. Some have thought that in his last public appearance at Oxford in 1894 he hinted his willingness to make a present of Darwin's theory to Lord Salisbury, as organic evolution could be established without it. And no doubt that is a view which can be maintained. Lord Salisbury had ridiculed the idea of the advantageous male in pursuit of the advantageous mate. This only showed that he could have studied Darwin to very little purpose. I am not one of those who think that the discontinuous "sport," advantageous or not, has played much part in evolution. But in any case its appropriate pairing is not essential, as it is now known that sports are frequently prepotent and their influence not easily swamped. The unmatched advantageous male is not so easily dismissed as Lord Salisbury seemed to think.

Huxley found in Darwin what he had failed to find in Lamarck, an intelligible hypothesis good enough as a working basis. But with the transparent candour which was characteristic of him he never to the end of his life concealed the fact that he thought it wanting in rigorous proof.

Now Darwin was a naturalist, and the "Origin" is emphatically the production of a naturalist. Huxley has repeatedly told us, what is perfectly true, that he was not one himself. "His love of nature had never run to collecting either plants or animals" (ii. 443). For him as for others Lyell "was the chief agent in smoothing the road to Darwin" (i. 168), for evolution is implied in uniformitarianism. Huxley was an anatomist, and the distinctions of the higher groups with which he chiefly occupied himself are anatomical. The discontinuity of those groups no longer troubled him now that he knew what lay behind Darwin's "humourous smile." But with "species" or primary groups he still found difficulties which I think he would not have found if he had had a naturalist's experience. At Edinburgh :—

"In common fairness he warned his audience of the one missing link in the chain of evidence—the fact that selective breeding has not yet produced species sterile to one another" (i. 193).

He states the point more precisely in a letter to Kingsley :—

"He (Darwin) has shown that selective breeding is a *vera causa* for morphological species; he has not yet shown that it is a *vera causa* for physiological species" (i. 239).

Now it seems to me that, to use one of his own favourite expressions, this is a shadow of the mind's own throwing. The species which Darwin undertook to account for are morphological. No other category conveys any meaning. There is a physiological difference between the sweet and bitter almond, because one is harmless and

the other will kill; but it is unaccompanied by the smallest morphological distinction. Nägeli pointed out the importance of recognising this in bacteriology. What Huxley really meant by physiological species are species which are mutually sterile, and in this both he and Romanes seem to me to have rather begged the question.

Darwin, who was more aware of the weak points of his theory than any of his critics, took immense pains to show that sterility does not run parallel with taxonomic order. It is well known that it is *not* a criterion of species, as Huxley seemed to think—it does not seem to be even a criterion of genera. I can only suppose that some hint of Huxley's furnished the foundation of Romanes's heroic attempt to establish "physiological selection." If so, Huxley seems to have been little impressed with the result:—

"It (the 'Origin') is one of the hardest books to understand thoroughly that I know of, and I suppose that is the reason that even people like Romanes get so hopelessly wrong" (ii. 192).

But then Romanes was not a naturalist either.

Another difficulty was the principle that "*Natura non facit saltum*" (i. 176), and I think from the same cause. Bateson, of course, receives a benediction:—

"I always took the same view, much to Mr. Darwin's disgust" (ii. 372).

That "considerable 'saltus'" may occur is not improbable; but there can be little doubt that a species passes from one configuration to another, as Darwin supposed, by minute changes; and, as he has himself pointed out, we are not justified in assuming that the rate of variation has always been uniform.

Huxley, however, felt that he had at last a secure grip of evolution, and was soon on the war path; he warns Darwin:—

"I will stop at no point as long as clear reasoning will carry me further" (i. 172).

Nor did he. The history of "the great 'Sammy' fight" has often been told. It is interesting to know that it was Chambers, the author of the "Vestiges," who was responsible for it (i. 188). Its importance has been somewhat exaggerated. Evolution has made its way by a process of slow permeation. It has done so because, in the words of Helmholtz, it contains "an essentially new creative thought" (i. 364). But it was a brilliant dialectic victory for Huxley, and Oxford loves dialectic: "The black coats . . . offered their congratulations" (i. 189). "The Bishop . . . bore no malice, but was always courtesy itself" (i. 188). Huxley was, however, less forgiving, and put him in his pet little Inferno (ii. 341). Personally I entertain more than a sneaking admiration for him. He "cleaned up" the diocese of Oxford with a vigour worthy of Huxley himself.

One incident in the discussion is of some theoretical interest. The permanence or, as I prefer to say, stability of species seem to have been adduced as an argument against Darwin's theory. Lord Avebury:—

"instanced some wheat which was said to have come off an Egyptian mummy, and was sent to him to prove that wheat had not changed since the time of the Pharaohs, but which proved to be made of French chocolate" (i. 187).

But we have absolute evidence from tombs that Egyptian plants have not appreciably changed for 4000 years. And it is now known that the fact, instead of being an argument against, is rather one for the Darwinian theory.

Owen made a last desperate attempt to save the situation by asserting for man, on anatomical grounds, a completely isolated position in the animal kingdom. Huxley, in 1862, "showed that the differences between man and the higher apes were no greater than those between the higher and the lower apes" (i. 192). The case for the evolution theory was now complete.

Carlyle did not forgive the publication of "Man's Place in Nature," though it only carried the veracity of "Sartor Resartus" a step further. However, master and disciple both received together an honorary degree at Edinburgh, and I think there must have actually been some sort of reconciliation. For I have a distinct remembrance of hearing, I think from Huxley himself, that Carlyle expressed to him unbounded admiration for "Administrative Nihilism," coupling it with a by no means flattering estimate of another eminent philosopher.

Here I must leave Huxley's scientific work. He was now only thirty-seven. He found zoology in this country enchained in fantastic metaphysical conceptions; he extricated it almost single-handed. Writing to Leuckart in 1859 he says:

"Ten years ago I do not believe there were half-a-dozen of my countrymen who had the slightest comprehension of morphology. . . . I have done my best, both by precept and practice, to inaugurate better methods. . . . I confidently hope that a new epoch for zoology is dawning amongst us" (i. 163).

The hope has been amply realised. And if a quickening spirit has been breathed into every branch of biological teaching in this country, it was Huxley it came from. It is much to be wished that some one would record some recollections of the memorable courses of instruction at South Kensington which Huxley commenced in 1871, in which teachers and taught were alike inspired by an enthusiasm the tension of which almost reached breaking point, and in Huxley's own case, in fact, speedily did so.

Notwithstanding ill health his mental activity, constantly stimulated by a certain innate combativeness, kept him to the end immersed in public work of the most varied description and in the controversy that he loved. "Under the circumstances of the time," he says, "warfare has been my business and duty" (ii. 213). All this it is needless for me to touch upon. But no picture of Huxley would be complete which left out of sight the speculations which more and more absorbed him as his life drew to a close. In this Journal these can be only treated from a purely scientific point of view.

It is necessary to remember that Huxley's grasp of the principle of organic evolution was only arrived at by the process of reasoned and by no means hasty conviction. He satisfied himself that man could not be excluded from it. He was naturally therefore drawn to discuss human phenomena in relation to evolution.

The first was the problem of ethics. He summed up his conclusions in the Romanes lecture delivered at Oxford in 1893. This was his second speech delivered there; the first was in the "great Sammy fight," thirty-three years before. He might well say that "Oxford always represents English opinion in all its extremes" (ii. 441). He nearly succeeded in producing as much hubbub as on the first occasion. It is amusing, if not very edifying, to read the anxious preliminary negotiations. Huxley wrote, "Of course I will keep clear of theology" (ii. 350), and Romanes naturally writes back "in great alarm" (ii. 354). The pith of the whole thing was, "the cosmic order is not a moral order."

Morals are part of the cosmic order, but not identical with it. Seriously regarded, this is a very simple statement of pure fact, which is indeed the basis of one of Dr. Watts's most familiar "Sacred Songs," the orthodoxy of which no one has ever impeached. The order of nature is self-regarding, and, as that familiar writer implies, society "would be dissolved by a return to the state of simple warfare among individuals" (ii. 352). The contrary view, embodied in the phrase "ethics of evolution," Huxley traces to the ambiguity of the word "fittest." That "which survives in the struggle for existence may be, and often is, the ethically worst" (ii. 303).

"The actions we call sinful are part and parcel of the struggle for existence . . . and have become sins because man alone seeks a higher life in voluntary association" (ii. 282).

So far this is a utilitarian theory of morals, and, as far as it goes, accounts for the phenomena. But, as Huxley saw, it leaves unexplained the fact that probably every ethical system aims at a higher standard than is ordinarily reached or is perhaps even necessary in practice. This apparently he would explain by "an innate sense of moral beauty and ugliness (how originated need not be discussed)" (ii. 305). I confess I am sorry for that parenthesis. But the principle itself is comparable to Matthew Arnold's "Something not ourselves which makes for righteousness." At any rate, short work is to made of those who do not possess it.

"Some are moral cripples and idiots, and can be kept straight not even by punishment. For these people there is nothing but shutting up or extirpation" (ii. 306).

I hope it is not irreverent to say that "Injuns is pisin" seems to be a natural corollary. Huxley meant to look up Nietzsche (ii. 360), but probably never did. Had he done so the result would have been edifying.

A critical study of Huxley's theological views, especially in the light afforded by the "Life and Letters," would be extremely interesting. This is not the place to attempt anything of the sort. But some brief account is necessary. The starting point is to be found in a letter to Kingsley:—

"'Sartor Resartus' led me to know that a deep sense of religion was compatible with the entire absence of theology" (i. 220).

Now this suggests two remarks which are both justified, I think, by my own personal knowledge. In the first place I am firmly persuaded that he, if any one, was a deeply religious man. I am equally persuaded that he had a perfect passion for technical theology. He often thought himself, at least so he told me, that he might have been a successful lawyer. I do not doubt it. But the cerebral equipment which might have found employment in that direction got turned on to theology. This, I think, throws light on his shortcomings in this field. Dogma may be treated, and I think should be, in a scientific spirit; Huxley too often indicted it as if he were in a police court. There is no doubt that he adopted this attitude deliberately.

"My object has been to stir up my countrymen to think about these things; and the only use of controversy is that it appeals to their love of fighting and secures their attention" (ii. 291).

"I must," he says, "have a strong vein of Puritan blood in me somewhere" (ii. 91), and I think it cannot be doubted that he was right. His point of view was that of an extreme nonconformist. I need not say that this implies no disrespect, for nonconformity has been one of the roots of the English character.

In one aspect the religious sentiment is a response to the craving for a supernatural sanction to rules of conduct. Its varied but practically universal manifestation amongst mankind has got to be accounted for by evolution just as much as the possession of a vertebral column. It is not practically helpful to dismiss it as irrational.

Huxley, like others of a Puritan temperament, had more liking for the Old Testament than the New: "the only religion that appeals to me is prophetic Judaism" (ii. 339). But Calvinism, I think, contained much with which he most nearly sympathised. "Science," he wrote to Kingsley, "seems to me to teach, in the highest and strongest manner, the great truth which is embodied in the Christian conception of entire surrender to the will of God" (i. 219). "I have the firmest belief," he continues, "that the Divine Government . . . . is wholly just." There is a very interesting passage, too long to quote (ii. 303), in which he points out that "the best theological teachers . . . . substantially recognise these realities of things, however strange the forms in which they clothe their conceptions." For my own part, I wish he had applied the principle which is implied here in some of his controversial essays. Writings thousands of years old would have been unintelligible if they had not been expressed not merely in the language but in terms of the ideas current at the time. The demonology of the first century was scarcely worth the powder and shot bestowed upon it. If it had cost Huxley himself "many a struggle to get free" from the Pentateuchal cosmogony (i. 167), he lived to see Canon Driver give up its "physical truth . . . . altogether" (ii. 218); the process of attrition of what is superfluous will go on.

Huxley, however, in his episcopophagous mood was a grievous disappointment to extremists when it came to practical business. It is difficult, I think, to exaggerate the importance of the work he did on the London School Board and at a terrible cost to his health. He expressed "his belief that true education was impossible without 'religion,' of which he declared that all that is unchangeable in it is constituted by the love of some ethical ideal to govern and guide conduct" (ii. 340), and he unhesitatingly adopted the words of Mr. Forster in 1870:—

"I have the fullest confidence that in the reading and

explaining of the Bible what the children will be taught will be the great truths of Christian life and conduct, which all of us desire they should know" (ii. 344).

He fought, therefore, "for the retention of the Bible, to the great scandal of some of my Liberal friends," and "never had the slightest sympathy with those who, as the Germans say, would 'throw the child away along with the bath'" (ii. 9).

Years after he remained of the same mind:—

"I do believe that the human race is not yet, possibly may never be, in a position to dispense with it" (ii. 300).

Ethical and religious problems occupied so large a place towards the end of Huxley's life that it was impossible to leave them out of sight. But a sharp distinction is, I think, to be drawn between what he accomplished in this field and what he did for knowledge. The latter was eminently constructive: he reconstituted biological science in this country from the foundations upwards. The former was only critical and, as he did not deny, mainly negative. His defence was that his part had been to clear "the ground for the builders to come after him" (ii. 301). Meanwhile he had nothing but respect for those who honestly held opposite views. But he would have nothing to do with the "half-and-half school," with whom he had less sympathy than "with thorough-going orthodoxy" (i. 471). For Magee, Bishop of Peterborough, he had "a great liking and respect" (ii. 244). I wish I felt at liberty to amplify what is said (ii. 205) as to the admiration he conceived for Father Steffens.

Looking back on the whole story as I have attempted to tell it, I am struck with the character of inevitableness about Huxley's career. I do not call to mind any other in which a controlling purpose so definitely manifests itself. "My sole motive," he said in 1891, "is to get at the truth in all things. I do not care one straw about fame, present or posthumous" (ii. 281), and certainly, so far as it is given to any one to be successful, he obtained a large measure of success.

Much has been said of the odium and obloquy he encountered in the process. He was certainly supremely indifferent to both, and probably rather enjoyed them. But Englishmen will concede anything to honesty, and Huxley was transparently honest. And obloquy is perhaps not intolerable which is accompanied by the repeated offer of a professorship at Oxford, followed by that of the headship of a college, by the presidency of the Royal Society, and by admission to the Privy Council.

But it was not merely as a man of science and of affairs that Huxley achieved success. He was possessed of an extraordinary literary gift. "I have," he writes, "a great love and respect for my native tongue, and take great pains to use it properly" (ii. 291). It is much to be wished that scientific men generally would follow his example. He could always, says Sir Spencer Walpole, "put his finger on a wrong word, and he always instinctively chose the right one" (ii. 25). But this, like everything else that he ever did, was not accomplished without labour. It was from the literature of the eighteenth century that young Englishmen "would learn to know good English when they see or hear it" (ii. 285). In his own case it helped to make him, as Mr. Arthur Balfour said, a great master of English prose; perhaps even, as Sir Spencer Walpole thinks, "the greatest master of prose of his time" (ii. 25).

Nor less sedulously did he cultivate the art of oral exposition and of public speaking, or with less success. Lord Salisbury exclaimed, "What a beautiful speaker he is" (ii. 25). Apart from eloquence as it is ordinarily understood, or rhetorical effect, I myself have never heard any one who in method or manner could compare with him. It is quite consistent with this that he should say, "I funk horribly, though I never get the least credit for it" (i. 311). Before one of his greatest performances he asked me to take his hand: it was stone-cold. "It is always like that," he said. Yet he held an enormous audience enchained while he unfolded, using no notes, but with faultless choice of words, an intricate and technical argument.

Nor was he less captivating in conversation. He rises to my mind's eye, drawing down his mouth when he was serious, as if to give momentum to the propulsion of the thought. In a moment, as some humorous aspect of the matter struck him, it would relax into a smile, and then, if one tried too audaciously to attack his arguments, his head would go back with a leonine sweep, as much as to say, "young man, be careful." But it was what Mr. Skelton admirably calls "the Shakespearian gaiety of touch" (ii. 16) that made converse with him so unforgettable. Darwin had something of it, but attuned to a gentler key. With Huxley it was irrepressible. "I suppose," he says, "I shall chaff some one on my death-bed" (ii. 76).

But, in truth, through these two volumes there runs a tragi-comedy, often moving to mirth and not seldom to tears, and sometimes almost Meredithian in intensity. The demon of dyspepsia broods over the drama, as it unfolds, like fate. The wonder is that a man who fought such a life-long battle with ill-health could oppose such a courageous and uncomplaining front to the outside world. He carried the fox gnawing at his vitals with a Spartan fortitude.

And to ill-health there was added, for no small portion of his life, the no less uncomplaining struggle with poverty. To keep his brother's widow he was even compelled to part with his Royal medal (i. 248). When he retired from the public service it was the desire of the Education Department that he should do so on a full pension. This the Treasury were unable to grant. But it is to be counted to the credit of a Tory Government that the amount was eventually made up from the Civil List.

A few words and I have done. In these volumes the reader has the privilege of being brought into as frank an intimacy with Huxley as was enjoyed by even his closest friends. I am wholly mistaken if there does not emerge from their perusal a personality of singular fascination behind which lay an intellectual and moral force, second perhaps to none in its influence on his countrymen during the latter half of the century which has closed.

As Lord Hobhouse has said, "he fought the battle of intellectual freedom" (ii. 407), and his success was due

to the integrity of purpose and dauntless courage which never failed him. Sir Spencer Walpole says justly,

"Of all the men I have ever known, his ideas and his standard were—on the whole—the highest" (ii. 27).

He proceeds—

"He recognised the fact that his religious views imposed on him the duty of living the most upright of lives."

A very unfair use has, I think, been made of this opinion, which I am persuaded is based on a profound misconception. However derived, it is in an innate sense of moral beauty that I prefer to find the true secret of Huxley's life. W. T. THISELTON-DYER.

TERRESTRIAL MAGNETISM AND ATMOSPHERIC ELECTRICITY.

The Norwegian North Polar Expedition, 1893–96. *Scientific Results.* Edited by Fridtjof Nansen. Vol. ii. (London: Longmans, Green and Co., 1901.)

Report on Observations in Terrestrial Magnetism and Atmospheric Electricity made at the Central Meteorological Observatory of Japan for the Year 1897. Pp. 60. (Tokio: Central Meteorological Observatory.)

THE first of the above volumes consists of three memoirs, numbered VI., VII. and VIII., written respectively by Prof. H. Geelmuyden, Mr. Aksel S. Steen and Prof. O. E. Schiøtz. In a brief preface Dr. Nansen states that the great majority of the observations dealt with were made by Captain Sigurd Scott-Hansen.

VI. *Astronomical Observations.*—In a preface, pp. vii. to lx., Prof. Geelmuyden describes the astronomical instruments and the circumstances of their use. His principal object is to determine the drift of the *Fram* and the track of Nansen and Johansen after leaving the ship. The results are embodied in two large scale charts (in a pocket at the end of the volume). A second object is to determine the azimuth in connection with the observations of magnetic declination.

The latitude and local time were found by altitude observations, the sun alone being available during part of the year. For the determination of longitude, and of the chronometer rates, a variety of data were accumulated. There were observations during two eclipses, a few lunar distances and a number of observations of eclipses of Jupiter's satellites. In connection with these last data there is an enumeration of corresponding observations at various observatories, and a discussion of the theory and of various sources of uncertainty. The differences between the chronometers in use from 1893 to 1896 are recorded and discussed. The difficulties met with in reducing the astronomical observations are considerable. Most referred to a station in motion, while many were taken at extremely low temperatures, under conditions when ordinary astronomical formulæ for refraction, &c., are open to question. The differences between the chronometers are not always easy to explain, and the data as to their temperature corrections are somewhat uncertain. As to the data obtained by Nansen and Johansen in their journey, in Prof. Geelmuyden's words,

"the observations during this expedition, where the principal work of the travellers was very often a struggle for life, and where the instruments had to be handled in temperatures down to $-40°$ C., with no other source of heat than the observer's own body, could not attain any high degree of accuracy" (p. lvii.).

The fact that the observations were made at all is the strongest possible evidence that scientific zeal is compatible with the possession of remarkable physical courage and resolution.

After Geelmuyden's preface follow tables, pp. 1–136, giving full details of all the astronomical observations, with a few explanatory notes.

VII. *Terrestrial Magnetism.*—In his introduction, pp. 1–9, Steen describes the instruments. Acknowledgment is made of the assistance rendered by Dr. Neumayer, of Hamburg, who selected the apparatus and had some of it made under his own eye. The great majority of the observations were taken on the ice, inside a tent or a house of snow or ice. "As a defence against bears . . . a weapon was always at hand, generally a revolver." The position of this useful but embarrassing auxiliary and its influence, or absence of influence, on the magnets is a frequent item in the observational records. The different magnetic elements are discussed separately. The declination observations occupy pp. 10–61. The majority were taken with a "Neumayer Declinatorium," of which the principal feature is that its magnet consists of "two laminæ, between which the mirror was fixed"; the magnet rested on a pivot, but could be inverted so as to determine or eliminate the collimation error. Declination results are also deduced from the deflection experiments, intended primarily for the determination of the horizontal force. There were in all about 130 days on which declinations were obtained. The *changes* observed during each of these days are shown graphically, occupying seventeen plates. The observations seldom extended over more than two or three hours on any one day, and in no case was there a continuous day's record. On November 24, 1894, in the course of fifteen minutes, the declination changed fully 26°. On no other occasion did the observed range exceed a quarter of this; but changes of 2° or 3° in the course of an hour or two were not uncommon.

The discussion of the horizontal force observations occupies pp. 62–126, the results being summarised on pp. 119–126. The apparatus was by Zschau. Observations of vibration and deflection were made much in the usual way. The moments of inertia of the two magnets used had been determined, but only approximately, and instead of employing the values so calculated use is made for each magnet of a "constant," involving the moment of inertia, which was determined by observations made at Hamburg and Wilhelmshaven. In some instances the horizontal force is deduced from a deflection experiment alone, by means of a second "constant" involving the magnetic moment of the deflecting magnet. The times of vibration were taken without a telescope, and no observations were made on the torsion of the silk suspension. Mr. Steen also experienced some trouble in connection with the temperature coefficients, which had not been determined at Arctic temperatures.

The inclination observations are discussed on pp. 127–165. The instrument used was a Fox circle, as

modified by Neumayer, with two needles and deflectors. In all there were ninety-two observations of inclination. In treating them, Mr. Steen encountered difficulties. In general, the magnetic axis of a dip needle is inclined to the line of geometrical symmetry, the position of which is read, while the C.G. departs from the axis of rotation. The former source of index error is usually eliminated by reading the needle with its face alternately towards and away from the face of the circle; whilst the latter source of error is removed by reversing the needle's magnetisation in the middle of each experiment. The needles, however, of a Fox circle are never reversed, and the observer in the present case had always used the needles in an invariable position. Assuming constancy in the magnetisation, the error from the first source would remain constant, but that from the second source would vary with the inclination. The resulting error is represented by Mr. Steen through a formula involving three unknown constants, but he finds the data insufficient for determining these directly. Eventually, by having recourse to some results obtained with the deflectors of the Fox apparatus and to corresponding values obtained for the horizontal force, and making certain assumptions, he arrives at numerical results. Some doubt may, however, be felt as to the measure of success attending Mr. Steen's courageous efforts, and this is the more to be regretted because the index correction applied averages about 50′.

The total intensity is dealt with on pp. 166–180. A considerable number of observations had been made with the Fox apparatus; the data, however, for converting these to absolute measure were not, in Mr. Steen's opinion, satisfactory. Accordingly, he contents himself with a list of the observational results,

"partly for possible future utilisation, and partly, too, to show what might have been done with the instrument if the necessary determination of the constants had been forthcoming" (p. 168).

Mr. Steen finishes with two tables, the first, pp. 183–188, summarising the individual results for the magnetic elements, along with the corresponding *theoretical* results, which Dr. Ad. Schmidt had the kindness to calculate from his values of the Gaussian constants for the epoch 1885. The discrepancies, which in the case of both the horizontal force and the inclination seem always of one sign, are often considerable. This may be partly due to the secular change, for which no allowance could be made. The second table arranges the observational results in groups.

VIII. *Pendulum Observations.*—Prof. Schiøtz in his introduction describes the apparatus, which consisted of a von Sterneck's outfit with two half-second pendulums. The periods were observed at Vienna, also at Christiania before and after the expedition, and Schiøtz concludes that practically no change had occurred. During the expedition one observation was made on shore near the Kara Sea, three on the ice, and seven inside the *Fram* when frozen in. Particulars of each experiment are given in full. The geographical coordinates of the stations and the corresponding times of swing are summarised on p. 55. To deduce absolute values for *g*, Schiøtz utilises the times of swing observed at Vienna and Christiania, together with the absolute values found for gravity at these places by von Oppolzer and himself. The values thus deduced for gravity at the polar stations are compared on p. 60 with the theoretical values given by Helmert's formula for *g*. Of the ten experiments taken on board ship or on the ice, five give values above and five values below the theoretical. The mean departure from the theoretical values, taken irrespective of sign, amounts only to three parts in 100,000; on the average the observed value exceeds the theoretical by one part per 100,000. Schiøtz believes, however, that the irregular movements due to ice motion must have slightly increased the observed values of *g*. His conclusion on p. 63 is as follows:

"The observations show that gravity may be regarded as normal over the polar basin; and as it is not probable that this is a peculiarity of the Polar Sea, we are led to the assumption that gravity is normal all over the great oceans. The increased attraction observed on oceanic islands must, therefore, only be due to the local attraction of the heaped up masses . . . that form the islands."

Prof. Schiøtz seems here rather a long way from his base. He devotes pp. 63–86 to drawing "some conclusions respecting the constitution of the earth's crust." A supplement, pp. 87–90, advances arguments which, in Schiøtz's opinion, justify the belief that the influence due to the lack of absolute rigidity in the supports on the pendulums' periods was the same throughout the voyage as at Christiania.

The writers of the three memoirs have clearly acted on the view that the circumstances of Nansen's polar journey were so unique as to justify an unusual amount of detail in recording the observations, and they have spared themselves no trouble in their anxiety to utilise the data to the very utmost. A critic may perhaps, however, be pardoned the doubt whether greater compression of details and greater reserve in theoretical deductions might not have led to a work of fully greater utility. Be this as it may, the volume is to be welcomed as exceedingly opportune in view of the approaching Antarctic expeditions. Those responsible for the exercise of foresight in connection with the apparatus, or the observational programmes of these expeditions, would be well advised in giving its contents their careful consideration.

According to the preface of the second work mentioned at the head of this notice, the Central Meteorological Observatory of Japan, at Tokio, was established in 1890, and "was rebuilt in July, 1897." (?) It possesses two underground magnetic rooms, one for photographically recording, the other for eye-reading, instruments. The former set of instruments are Mascart magnetographs, the latter are said to be of a similar type. The instruments for absolute observations are illustrated in a plate at the end of the volume. The declination and horizontal intensity are observed with an instrument due to Prof. Tanakadate, possessing some unusual features, of which a fuller description is given in the *Proc. R.S.E.* for 1884–6. The times required for taking the several observations are given as: declination, 5 minutes; horizontal force, 20 minutes; inclination, 20 minutes! Absolute observations are taken on only one day a month, but the operations are repeated

several times; there are also monthly determinations of the curve scale values. The necessary temperature corrections are based on direct experiments on one occasion when the magnetograph room was artificially heated. Hourly measurements are made of all the magnetic curves and the results appear in tables, one for each element for each month. In addition to the hourly readings, each table gives the daily mean, maximum, minimum and "range" (maximum less minimum); it gives also the monthly mean for each hour of the day, and the means for the month of the diurnal maxima, minima and "ranges." The general character of each day, whether quiet or more or less disturbed, is also noted. The monthly means are summarised on p. 59, and there are curves of diurnal variation for each month and for the year.

The mean monthly diurnal variations are also analysed in Fourier's series. The fact should be noted that the "mean range" of any element for each month being the mean of the differences between the daily maxima and minima, irrespective of the times of their occurrence, is necessarily larger than the range shown in the mean monthly diurnal variation; it may be largely influenced by the occurrence of magnetic disturbances. For instance, the "mean range" in the horizontal force is given as greater in December than in any other month except April, but the value in December is considerably affected by the occurrence of two or three exceptionally large "ranges." The mean monthly diurnal variations are less exposed to accidental influences; their nature is most easily followed in the curves. The amplitude of the regular diurnal variation in both declination and horizontal force appears least in November. The amplitudes in January appear surprisingly large compared to those in the last three months of the year.

The atmospheric electricity installation consists of a Kelvin water-dropper, the discharge tube of which projects "about 2 metres" at a height of 1·7 metre above the ground. This is connected to the needle of a quadrant electrometer, the quadrants of which are connected the one pair to the positive the other pair to the negative pole of a battery of water cells, the centre of which is to earth. This seems the same arrangement as at Kew. The scale value of the curves is determined weekly. The hourly readings are recorded in tables, one for each month. The daily means, maxima, minima and "ranges" are recorded as in the magnetic tables, also the nature of the daily weather. Hourly means are also given for each month, but in forming these a considerable number of individual results are omitted as being abnormal. Amongst the values omitted are most of the negative potentials, and some entire days are excluded on which negative readings were numerous. The measurements of potential being all given in volts, one can follow readily the annual change, which is more than usually pronounced. Thus the mean potential for the year being 47·2 volts, the mean voltage for the three months December to February was 93·2, while that for the three months July to September was only 9·0; the maximum monthly mean was 112·8 in December, the minimum 6·9 in August. It may be added that the exceptionally low value in August appears in no way due to exceptional occurrences of negative potential.

The mean diurnal variations for the several months are illustrated by curves. As usual there is a marked tendency to a double diurnal variation; but the principal maximum occurs between 6 and 8 a.m., instead of, as is customary, in the late evening. Further, the principal minimum is found, the whole year round, in the early afternoon, usually from 2 to 3 p.m. These results possess a special interest from their apparent irreconcilability with views due to Chauveau which have recently met with considerable recognition.

Another notable feature is the size of the mean diurnal variation. The largest mean hourly value of the potential is in most months some four or five times the smallest; for instance, amongst the mean hourly values in December the maximum was 187·6, the minimum 42·1, while the corresponding values in August were 13·7 and 2·8. The peculiarities shown by the atmospheric electricity results at Tokio are, to a considerable extent, manifested by observations made during a series of years in the Batavia Observatory, but though Batavia is much nearer to the equator than Tokio, the results at the latter station show the greater departure from those ordinarily recorded in Europe.

What has been said will suffice to show the interest of the volume, and the evidence it affords of the progressive spirit in Japanese science. The continuance of the observations, and also of the practice of describing them in English (sometimes with Japanese equivalent), is much to be desired. There are, however, one or two points where some friendly criticism may be offered. The variation in the scale value of the horizontal force curves —from $10^{-6} \times 69$ to $10^{-6} \times 57$ C.G.S. units for 1 mm.—is excessive; and the vertical force scale values show even larger variations (1 mm. $= 10^{-6} \times 82$ in April, $10^{-6} \times 239$ in June, and $10^{-6} \times 79$ in November). Changes such as these, unless produced designedly at known times, introduce uncertainties into at least the annual variation in the amplitudes of the diurnal inequalities. Again, the temperature coefficients are so large for both the horizontal and vertical force magnetographs that appreciable uncertainty must be introduced into the diurnal variations unless the changes of temperature are known with extreme accuracy. Under such conditions, the employment of *six* significant figures in the tables of monthly means of the diurnal variations of the force components seems hardly well advised.

In the case of atmospheric electricity, 1 mm. of curve ordinate answered, on the average, to 3·86 volts in February, 1·15 in August and 2·69 in December. If, as one would rather *infer* from the preface, the number of battery cells in use varied from 30 to 50, one would not be surprised at a considerable change in the scale value, and it would be only good policy to have the scale contracted in winter when the mean potential is large. The changes recorded in the scale value seem, however, too large to be wholly accounted for in this way, and they do not exhibit so regular a fluctuation as to suggest design. A little further information on these points would enable the critic to pronounce with greater assurance on the value of the results.

C. CHREE.

STORAGE RESERVOIRS.

Reservoirs for Irrigation, Water-Power, and Domestic Water-Supply. By James D. Schuyler, M.Am.Soc. C.E. Pp. xviii+414. (New York: John Wiley and Sons. London: Chapman and Hall, Ltd., 1901.)

THE title-page of this book states that it also contains "an account of various types of dams, and the methods and plans of their construction; together with a discussion of the available water-supply for irrigation in various sections of arid America; the distribution, application, and use of water; the rainfall and run-off; the evaporation from reservoirs; and the effect of silt on reservoirs." The book was, accordingly, designed to embrace all the main questions relating to the construction of reservoirs, together with the distribution and use of the water stored up; but in reality the different methods of construction of reservoir dams, descriptions of numerous examples in the United States, and references to the works required for several projected reservoirs constitute the principal subjects dealt with. The most remarkable feature, however, of the book is the abundance of views of reservoir dams, reservoirs, and proposed sites for reservoirs, comprising a large proportion of the one hundred and eighty-three illustrations, which should prove very attractive to the general public; whilst the plans and sections of dams and other contingent works, maps of reservoirs and of proposed sites for reservoirs with contour lines, and twenty-five folding plates, in an appendix, of reservoir sites in California and the Lahontan and Arkansas River basins, and of the Sun River system of reservoirs in Montana, will appeal mainly to engineers.

The book is divided into only six chapters, treating respectively of Rock-Fill Dams, Hydraulic-Fill Dams, Masonry Dams, Earthen Dams, Natural Reservoirs, and Projected Reservoirs, to which an appendix is added containing particulars of reservoir surveys and designs in California, Nevada, Colorado, Montana, Utah, New Mexico, and Arizona, and the cost of reservoir construction per acre-foot in the United States and other countries.

Rock-fill dams of a temporary character, formed of timber cribs filled with stone, were originally used in California for impounding water for mining purposes; and subsequently more serviceable and more watertight dams were obtained by introducing some dry stone walling in front of a loose stone embankment, faced with two or three thicknesses of planks. Since then the loose stone embankment has been made more durable by facing it with asphalt concrete, or Portland cement concrete, or steel plates, laid on a sloping dry wall, or by introducing a central core of steel plates, or by a facing of masonry backed with earth, or by facing it with earth. Examples of these various types of rock-fill dams are described in the first chapter; and the extent of irrigation effected by means of the water stored up by these dams is indicated. Naturally dams of these economical types, imposed sometimes by the inaccessibility of the site and the necessities of the case, and occasionally very carelessly constructed, have not been exempt from failures, their bursting having been sometimes accompanied by disastrous results.

In a few instances, reservoir dams have been formed in the United States by directing a powerful jet of water against the upper slopes of a valley, and thus causing the materials scoured from the hillsides to be conveyed by the water to the site of the dam proposed to be constructed across the lower part of the river valley. By suitable arrangements, the stream of water from the issuing jet both conveys the materials by gravity to the required site, depositing them along the lines of the two slopes which are kept higher than the centre of the embankment, and consolidates these materials in position, the larger stones being dropped at the sides, and the finer materials being carried towards the centre of the dam in drawing off the water through standpipes. The best materials for this hydraulic-fill construction are a mixture of soil, sand, and gravel of various sizes; and examples of dams in the United States constructed successfully by this method are given in the second chapter. Both rock-fill dams and hydraulic-fill dams exhibit the peculiar resource and boldness of American engineers; though Canadian engineers have resorted to the hydraulic system for the formation of permanent embankments on the Canadian Pacific Railway, in place of the temporary wooden trestle viaducts provided at the outset for crossing valleys and gorges rapidly and economically.

The conditions of stability of masonry dams, involving a solid rock foundation, and a well-established profile in section varying with the height, have been so fully recognised for many years past, and any considerable departure from them appears so certain to result in failure, as illustrated by the history of the Bouzey dam in France, which gave way in 1895, that there might seem to be little scope for novelty in such constructions. Whereas, however, in European practice the curvature of a masonry dam in plan has generally been merely regarded as conferring an additional element of stability on the dam, American engineers have not hesitated to rely largely on the arched form for the stability of some dams, which have been given such slight sections that they could not possibly have resisted the water pressure unaided. This is exemplified to some extent by the slender Sweetwater dam, 90 feet high and only 46 feet thick at the base, and curved to a radius of 222 feet; and more especially the Bear Valley dam, which, though only 64 feet high, has been made unprecedentedly slight with a thickness of only $8\frac{1}{2}$ feet 48 feet down from the top, where it rests on a masonry base 13 feet thick, so that its section is absolutely at variance with correct principles, and it would long ago have been swept away had it not been curved up-stream with a radius of 335 feet. The Zola dam in France was constructed about 1843, twenty-three years before French engineers inaugurated the correct profile for masonry dams by the completion of the Furens dam with ample stability in 1866, though retaining a maximum head of water of 164 feet; but unlike this latter dam, the Zola dam owes its stability entirely to its arched form in plan of 158 feet radius, coupled with the very short length of 23 feet at its base; for the Zola dam, though 120 feet high and 19 feet thick at the top, is only 49 feet thick at its base, showing that no approximation to the correct section had been reached at that period for what the author calls "gravity dams," supporting the water pressure by their weight alone.

Several examples of masonry dams in the United States are described and illustrated by views, sections,

and plans; whilst short references are made to the most notable masonry dams in other countries. La Grange dam in California, for diverting the water of the Tuolumne River for irrigation, 125 feet high, resembles the Vyrnwy dam in section, the outflow in both cases taking place over the top of the dam. The San Mateo concrete dam in California, designed to have a height of 170 feet, but stopped at present at 146 feet, and a total length at the higher level of 680 feet, has a bottom width of 176 feet, and is arched up-stream with a radius of 637 feet; and the reservoir formed by the completed dam will have a capacity of 29,000 million gallons. The Ash Fork steel dam, 184 feet long and 46 feet high for a central 60 feet, built in 1897 across Johnson canyon in Arizona, is a novel type of dam, constructed with triangular steel frames covered with steel plates; but the experiment has not proved satisfactory, as the steel dam leaks considerably at its junctions with the masonry buttresses at both ends, and with the concrete foundation at the base. An interesting form of the failure of a masonry dam is furnished by the history of the Austin dam in Texas, illustrated by views, 1091 feet long and 68 feet high, built in 1891–92 and founded on limestone rock. In April, 1900, an unprecedented flood of the Colorado River raised the water-level of the reservoir 11 feet above the crest of the dam; and 500 feet of the dam slid forward on the foundation about 60 feet down stream, though a flood in the previous summer, raising the water $9\frac{1}{3}$ feet above the crest, had passed down without injuring the dam. Another interesting feature of this work was the filling up of over two-fifths of the reservoir capacity with sand and silt in four years, owing to the yearly discharge of this sediment-bearing river amounting to about forty times the capacity of the reservoir.

Some earthen dams constructed in California and Colorado, for forming reservoirs for irrigation, are described in a short chapter. Natural reservoirs in the great plains to the east of the Rocky Mountains, formed by depressions collecting the storm waters from the adjacent districts and devoid of an outlet, can be readily utilised for irrigating arable lands at a lower elevation; and examples of such reservoirs are described in the fifth chapter. The final chapter is devoted to schemes for reservoirs, mainly in California, Colorado, Montana, New Mexico, and Utah, and like the preceding chapter possesses mainly a local interest; but the descriptions serve to show what a field there is in these Western States for such works, and what a large development of irrigation, with its attendant benefits, may be accomplished in these regions.

OUR BOOK SHELF.

The Anatomy of the Cat. By Jacob Reighard and H. S. Jennings. Pp. xx + 498. (New York: H. Holt and Co., 1901.)

YET another book upon the cat! With the great treatise of Strauss-Durckheim, and the books of Mivart, Wilder and Gorham, published, and the great work of Jayne in course of publication, there would seem little room left for this now before us. When, however, it is remembered that the treatise of the first-named author is not available for American students; that, like that by Wilder, it deals only with parts of the animal described; that the late Dr. Mivart's book, rather a general treatise on mammalian morphology than a special one upon the cat, fails completely in most parts where anatomical detail peculiar to this animal is concerned; that the book by Messrs. Gorham and Tower, though a laboratory treatise, is but brief—it will be clear that ample room is left for the work under review, which is designedly a laboratory book, giving a complete and well-balanced description of the facts of anatomy of the animal concerned "in moderate volume and without extraneous matter."

There are in all 472 pp. in the book, o. which the appendix of 44 pp. is wholly given to directions for practical dissection. The body of the work consists of brief but concise descriptions of the organic systems taken in order—the skeletal, muscular, visceral, circulatory, nervous and sensory systems (the latter with the integument) being in turn dealt with. Anatomical characters are alone recognised; neither those histological nor which concern growth stages of even the bones are in any way given; nor is there any reference to literature beyond brief mention of the works by the aforenamed anatomists and some few others, together cited in the preface. Our authors have done well to consult the myological observations of Windle and Parsons, but they have omitted to even record the important work upon the morphology of the digestive tract of the cat, by Dr. Franklin Dexter, of the Harvard Medical School, which has been progressing side by side with their own.

This book is what it professes to be—a laboratory treatise, clear, deliberate and clean cut, in its style and method most nearly akin to the didactic laboratory treatises of the late Milnes Marshall, so fully in vogue by the type of student who cares only for facts. It is based upon an earlier account of the anatomy of the cat, designed by the senior author for class use in the University of Michigan in 1891–92. The junior author is responsible for its completion for publication, and the 173 text illustrations, which, though clear, are in no way remarkable, have been prepared under his supervision by his wife.

The chief novelty of the book is a system of nomenclature, based upon that proposed in 1895 by the German Society of Anatomists. A large section of the preface is devoted to a discussion of this and cognate subjects; the use of Latin terms in their English form, and the significance of topographic terms and terms of precise orientation, being among the more important topics discussed.

We are informed that the notes which furnished the basis of the book have been used with success in four or five of the American Universities, and although among English teachers, who prefer the rabbit to the cat for educational work, the book will be little in demand, it will be welcome beyond those upon the cat hitherto in use on account of its accuracy of descriptive detail and uniformity of treatment.

Essays in Illustration of the Action of Astral Gravitation in Natural Phenomena. By William Leighton Jordan, F.R.G.S., M.R.I., Assoc.Inst.C.E., F.S.S., F.S.A., F.R.M.S. Pp. xv+192. (London: Longmans, Green and Co., 1900.)

WHEN an author puts forward perfectly new views in opposition to those generally accepted, using technical terms like *force* and *energy* in several new senses, it is very difficult to find out exactly what he means. In his definitions he says that gravitation resists all impressed motion with a force as the square of the velocity. He defines *vis inertiae* as the force with which matter resists motion. It is as the mass multiplied by the square of the motion resisted. After defining momentum, he says that it is resisted by the inertia of matter in its origin and in its progress, whereas Newton's first law of motion supposes inertia to resist its origin but to sustain its progress. The author's membership of many learned societies might warrant the belief that he has some meaning in what he says, but it is certainly very carefully concealed.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The National Antarctic Expedition.

I HAVE recently been made acquainted with certain hypotheses which are believed to explain the motives which induced Prof. Gregory to resign the position of scientific director of the National Antarctic Expedition. Thus, it is commonly believed that he was influenced by his family and friends. Indeed, the opinion has recently been expressed that I was, perhaps, the cause of his withdrawal, or that, at least, I advised it. It is impossible to imagine how such an opinion can have arisen if my letter to the Fellows of the Royal Society had been read with any attention, unless, indeed, I have failed to give a fair and accurate account, in spite of most serious efforts, put forth with a grave sense of responsibility.

I am, however, now able to set the matter at rest by a quotation from Prof. Gregory's letters received since the circulation of my account of the negotiations. I am quite sure that Prof. Gregory would have no objection to this use of his words in order to confront the unfounded rumours which have obtained currency.

It may be remembered that after the meeting of the Joint Committee on March 5, at which Major Darwin's proposed changes in the conditions offered to, and accepted by, Prof. Gregory were approved, although I had strongly opposed the introduction of any alteration whatever, "I wrote to Prof. Gregory a full account of what had happened, carefully explaining that his representative and many of his friends supported the changes, that I had confidence that the proposal was made to enable the Geographical Society to accept the instructions, and that it was not intended to prevent, and, I believed, would not prevent, his being landed" (p. 6 of my letter).

I have now received two letters from Prof. Gregory, one written on April 16, before he had received mine, the other on April 23, after he had received it.

In the former he says: "I hear that the Joint Committee has accepted some of Darwin's amendments; but as I do not know what they were I can form no opinion. But ——, —— and —— say they make no difference. I hope not."

In the latter, written in reply to my letter, he says: "Very many thanks for your fight against Darwin's amendment, which I should not have accepted had I been in London or been advised of it by cable. However, I suppose it is now too late to go back on it; and as it has [been] accepted for me I must trust to luck."

Later on in his letter the explanation of his resignation becomes perfectly clear; indeed, he asks me to make it known. In the event of the President of the Geographical Society declining to sign the instructions, he says: "*Please let it be known that, except for a modification backward of Darwin's amendment, I will not accept another change.*"

Between my letter describing the meeting on March 5 and May 15, when his final resignation was known, I held no communication of any kind with him. But others had communicated those further changes which he was determined not to accept.

It must be clear to any one who will read the history of the negotiations carefully, that he thought, and had good reason to think, that he was being trifled with, and felt that the time had come—to a less patient man it would have come long before—when he would no longer submit to the vigorous attacks of the Royal Geographical Society and the weak, half-hearted defence of the Royal Society.

* * * * *

A few hours after the above words were written a letter arrived from Prof. Gregory dated May 5, just after he had received the cable from the new Committee of six. The letter indicates clearly the reasons which induced him to withdraw, and I therefore quote several passages from it. The letter was written hurriedly, and not intended for publication; but I know that Prof. Gregory would assent to my action, pursued as it is with the object of preventing the misinterpretation of his motives. A few unimportant verbal changes have been made.

"You at least," he says, "will not have expected me to accept the cabled terms. I was not surprised at them; only surprised that the Royal Society had given way apparently so readily and that I heard the result a month earlier than I expected."

"The terms proposed appear to me, as far as I understand them from the cable, a complete surrender of what the Royal Society's representatives declared in February was essential to the proper execution of the magnetic work. The position gives no power to secure a fair opportunity for work to the man who would have to bear the blame for scientific failure."

"To accept responsibility without adequate power is a false position which is almost sure to lead to trouble. No man has a right to take such a position. As I do not think the powers are adequate to the responsibilities, it is my simple duty to withdraw. I hope the Royal Society will find a better man, who will be satisfied that he can make the Expedition a scientific success on the instructions given. I am not; therefore I must withdraw my provisional acceptance of the appointment."

"It will be difficult to prevent my withdrawal being misinterpreted. I had thought of cabling to ask you to publish an explanation, but thought it best to leave you to act as you thought best. I can absolutely rely on your judgment, and know you will have done anything necessary to repel insinuations."

I have done my best to prevent Prof. Gregory's motives from being misunderstood, and it is with the same object that this communication is now written and accompanied by quotations from his letters.

He concludes with a reference, which is far too appreciative, to the support which—unfortunately for the scientific prospects of the expedition and, I must add, unfortunately for the credit of the Royal Society as the guardian of the interests of science—received, at the later stages of the negotiations, the help of so small a proportion of my colleagues.

Oxford, June 11. EDWARD B. POULTON.

A Raid upon Wild Flowers.

Prof. L. C. MIALL, in the last number of NATURE, makes very definite and serious charges against the organisers of the vacation course for Essex teachers in the New Forest. As author of the programme so severely, and, as I contend, unfairly, criticised by your correspondent, I should be glad to be allowed an opportunity for reply.

The programme, as you will see by the copy enclosed, consists of two parts, the first dealing with a series of Saturday afternoon botanical rambles in our own county and the other with the proposed vacation course to be held at the New Forest. The first is of a pioneer character, and is open to all teachers whether they are familiar with botany or not, while the vacation course is organised for those of our teacher-students who have already received one, two or more years' instruction in laboratory and field-work in botany at the central institution here. For this course special application must be made to the committee.

From a perusal of the programme Prof. Miall accuses the Committee for Technical Instruction in Essex with organising a raid in the New Forest especially upon wild flowers tending to extinction, and bases his charge upon certain alleged facts. Your readers are told that with respect to these rare plants our intention is to collect, &c., "not only single specimens but duplicates for special fascicles." There is no such reference in the programme of the vacation course in the New Forest, but in a note at the end of the Saturday afternoon programme occurs these words.

"Opportunity might be taken, during the course of the Saturday rambles, of commencing a school herbarium, or collection of dried plants illustrative of the flora of collector's own district. A type collection would naturally be arranged in botanical order, but duplicates might be used for special fascicles representing, for example, 'meadow plants,' 'cornfield weeds,' &c."

The letter continues—"Local guides are to direct them to the last retreats of the rare plants of the New Forest." This, too, is a mistake. In the Saturday afternoon rambles we are to be accompanied by local guides whose names and addresses are given in the programme, but no such arrangements were made for the New Forest. It is true that I sought the sympathy of local naturalists, and, indeed, so anxious was I to prevent even the suspicion of "raiding" that I wrote to the Rev. J. E. Kelsall, the local representative of the Selborne Society, whose strong views on the preservation of the plant and bird life of the New Forest are so well known, to tell him of our proposal and to assure him that our chief object was the study of living plants, and that if we discovered anything rare, or even scarce, it would be left untouched by our students; and I thought that

the publication of the fact that Mr. Kelsall and Mr. Dale, secretary of the Hants Field Club, might be able "to accompany the party on one or more of its rambles" a sufficient guarantee that the rights of wild plants would be respected.

Furthermore, on the title page of the vacation course programme, p. 9, and printed in conspicuous black type, is the following notice: "Members of the party will, of course, refrain from uprooting rare or scarce specimens." Yet Prof. Miall alleges "there was no such restriction in the printed programme"!

In the daily itinerary as printed in the programme reference is made to the character of the scenery, the soil and surface geology, the prevailing vegetation, and to some of the rare plants growing in the neighbourhood. From what we have already shown it could hardly be our intention to raid these rare plants, and especially as several of those mentioned will be out of flower in August. Indeed, so particular are we in these rambles that the needless uprooting even of the commonest weed is discountenanced, as may be seen in the further notice on p. 3 of the programme.

In comparison with such a particularly odious charge as plant extermination, the other strictures of your correspondent's letter are, of course, scarcely worth noticing; yet even with respect to these I cannot resist pointing out that Prof. Miall's statements are strangely at variance with the actual facts. For example, he writes: "It is enough to condemn the programme as an educational project that novices knowing little or nothing of field-botany are set to study the subspecies of brambles." But does the programme so recommend? It distinctly says in reference to this (p. 18), that "their identification will give capital exercise in critical observation to the *more advanced worker*."

The real object of these field-studies, as stated on the front page of our programme, is to give teachers "an insight into the way in which plants grow, especially in their relations with their environment—the influence of external conditions, such as light, heat and moisture, upon their form, the mutual relationships between plants and animals and the influence of one organism upon another," and is in no way connected with collecting in the sense used by Prof. Miall. The vacation students have varied interests—flowering plants, algæ, leaf-fungi, &c., and the evenings are to be spent in discussing "the most interesting of the objects collected" and on the "preservation" of such as may be useful for class-work in the winter courses. Readers of NATURE will understand that work of this sort does not mean the collection of rare flowering plants.

Perhaps because of the peculiar gravity of the charge I may, in conclusion, be allowed to introduce one personal note into the reply. I should like to say that although I have conducted field studies in botany for the last twelve years (including two summer courses at the New Forest), yet, as it happens, I am no collector myself, and have never made what botanists would call a collection of dried plants in my life. Furthermore, I have never possessed, or even "coveted," a single specimen of a rare British plant. On the contrary, my sympathies are, of course, entirely with those who are opposed to any interference with our native flora, and I do most strongly protest against this attempt of Prof. Miall to connect in any way whatever our botanical work with such objectionable practices.

I should be glad to send a copy of the programme to any one who may care to see it. DAVID HOUSTON.

County Technical Laboratories, Chelmsford, June 10.

Emanations from Radio-active Substances.

IN a recent number of the *Comptes rendus* of the Paris Academy (March 25) an account appeared by MM. P. Curie and A. Debierne of the production of a radio-active gas from radium. In their experiments some radium was placed in a glass vessel and the air exhausted by means of a mercury pump. It was found that the vacuum steadily decreased, due to the giving off of a gaseous substance from the radium. A small amount of the gas thus collected was found to be strongly radio-active. It caused phosphorescence in the glass tubes over which it passed, and in course of time blackened them. Substances exposed in the gas became themselves temporarily radio-active.

Some time ago (*Phil. Mag.*, January and February 1900) I showed that thorium compounds continuously emitted radio-active particles of some kind, which preserved their radio-activity for several minutes. This emanation possessed the remarkable property of causing all bodies, in contact with it, to become themselves radio-active. In an electric field the excited radio-activity could be concentrated and confined to the negative electrode. In this way I was able to make a fine platinum wire become a very powerful source of radiation.

The excited radio-activity gradually diminished, falling to half its value in about twelve hours. The specimen of impure radium then in my possession gave out no emanation and caused no excited radio-activity. Later, Dorn, using the same methods, showed that a preparation of radium from P. de Haen, Hanover, gave out an emanation similar in properties to thorium. With a specimen of radium obtained from the same source I have found that the emanation given off is small at atmospheric temperature, but can be enormously increased by slightly heating the radium. In this way I have obtained ten thousand times the amount of emanation given off at ordinary temperatures. An account of these experiments is given in the *Physikalische Zeitschrift* (April 20).

By passing the emanation with a current of air into a closed vessel, and then closing the openings, the emanation remains radio-active for a long time. The radio-activity decreases slowly, but is still quite appreciable after an interval of one month. M. and Mme. Curies, some time ago, stated that they had obtained a radio-active gas which preserved its activity for several weeks; this is possibly identical with the emanation.

Up to this point I had been unable to obtain any definite evidence whether the so-called emanations were vapours of the radio-active substances, radio-active gases, or radiating particles large compared with a molecule. The radium and thorium, when placed in an exhausted tube, gave no appreciable lowering of the vacuum, and no new spectral lines could be observed. The quantity of substance emitted was too small to examine by chemical methods.

Quite recently, however, some light has been thrown on the question of the nature of these emanations by examining their rate of diffusion by an electrical method. In these experiments I have been assisted by Miss H. T. Brooks, and the results point to the conclusion that the emanation from radium is in reality a radio-active gas, with a molecular weight probably lying between 40 and 100.

There is one distinct feature which distinguishes the emanations from radium and thorium. The thorium emanation loses its radio-activity in a few minutes, while the excited radio-activity due to it lasts several days. The radium emanation, on the other hand, preserved its radiating power for several weeks, while the excited radio-activity due to it disappears in a few hours. In the following experiments it was only possible to experiment with radium emanation, on account of the rapid decay of radio-activity of the thorium emanation.

The diffusion apparatus was similar to that which had been employed by Loschmidt in 1870 in his determinations of the coefficients of interdiffusion of gases.

A brass cylinder, 73 cm. long, 6 cm. in diameter, was divided into two equal parts by a metal slide, which could be opened or closed. The ends were closed by insulating ebonite stoppers, through which passed central rods half the length of the tube. In order to introduce the emanation into one half of the cylinder the slide was closed, and a slow current of air, which had passed over slightly heated radium and thus carried the emanation with it, was passed through the cylinder. When a sufficient amount had been introduced the current of air was stopped and the openings closed. After standing for an hour or more the slide was opened, and the radio-active emanation slowly diffused into the other half of the cylinder. The amount of emanation in each half of the cylinder after any interval was tested by observing the current through the gas, when a suitable P.D. was applied, by means of an electrometer. The current is carried by the gaseous ions which are continually produced by the radiation from the emanation. From these observations the coefficient of inter-diffusion of the emanation into air at atmospheric pressure and temperature can be readily deduced. The experiments are, however, complicated by the excited radio-activity on the electrodes, which must be taken into consideration.

So far as the observations have gone up to the present, the coefficient of diffusion of the emanation into air has a value between 0·10 and 0·15, and probably nearer the former. Now the coefficients of inter-diffusion of some known gases and vapours

into air have been determined. The following examples have been taken from Landolt and Bernstein's tables:—

Gas or Vapour.	Coefficient of Diffusion into Air.	Molecular Weight.
Water vapour	0·198 ...	18
Carbonic acid gas	0·142 ...	44
Alcohol	0·101 ...	46
Ether	0·077 ...	74

In the above we see that the coefficients of diffusion follow the inverse order of the molecular weights. In cases of the simpler gases it has been shown experimentally that the coefficient of inter-diffusion is approximately inversely proportional to the square roots of the product of the molecular weights. If we apply these considerations to the emanations we see that it is a gas or a vapour of molecular weight (allowing a wide margin) probably lying between 40 and 100. These numbers exclude the possibility of the substance being a vapour of radium, for it has already been shown by M. and Mme. Curie that the atomic weight of radium is greater than that of barium.

We must, therefore, conclude that the emanation is in reality a heavy radio-active vapour or gas.

On account of the rapid decay of the radiating power of thorium emanations it is not possible to determine its coefficients of diffusion in the same way; but special experiments show that it diffuses rapidly, and is also probably gaseous in character. The physical properties of these emanations or gases are most remarkable. The radium emanation not only continues for long intervals to be a source of radiation which is apparently similar in character to easily absorbed Röntgen rays, but in some way manufactures from itself a positively charged substance, which travels to the negative electrode and becomes a source of secondary radio-activity.

Space is too short to enter into the interesting question of the possible explanation of these complicated phenomena.

McGill University, Montreal, May 30. E. RUTHERFORD.

Long-tailed Japanese Fowls.

A LITTLE while ago in your columns Prof. Lankester referred to this breed as "a magnificent sport," and considered the occurrence of genius in mankind as a case of the same kind. In Newton's "Dictionary of Birds," article "Feather," it is stated that in these Japanese poultry the moult is checked or prevented by some means unknown to Europeans. It is obvious that the latter statement, if correct, is not compatible with Prof. Lankester's description. If the breed really arose as "a magnificent sport," I presume that the excessive growth of the tail coverts would be due to a spontaneous variation, and not to some artificial method of preventing the annual moult. After a great deal of trouble I have succeeded in obtaining evidence, which seems to me unimpeachable, concerning the means taken by the Japanese to produce this extraordinary elongation of feather in the cocks of the breed in question.

I will quote the words of my informant. He writes:—"With regard to the treatment of these birds, in order to ensure very great length of tail, they ought after they are six months old to be kept on a perch as much as possible, and the tail feathers should be pulled gently every morning, grasping the centre bone-like part firmly with the finger and thumb, and, pressing steadily, draw downwards towards the tip, each feather being done several times; this softens the quill and causes it to lengthen. They do not moult the feathers, but if one or more come out others immediately grow in their place. The Japs themselves, those who take great pride in their birds, always roll the long feathers up, like a lady rolls up her hair, and tie them, whenever they are let off their perches to walk about, which is about twice a day for an hour at a time. . . .

"I have often seen them thus treated in Japan, and the man who brought mine over treated them in this way on the voyage over, and I sent them (to purchasers) in their regular perch cages."

I think this, being the evidence of direct observation, is enough to prove that the length of feather in these birds is not correctly described as a "sport," but has been produced by special artificial treatment. The effect of the treatment is doubtless to irritate the papilla from which the feather grows, and so cause increased growth, rather than to soften and lengthen the already formed quill. The feathers appear to grow throughout the year, so that when the moulting season is reached they are not shed, but continue growing.

There is no doubt that the peculiarity is to a certain extent hereditary, but extreme length of feather cannot, I believe, be produced without the special treatment. These fowls have been bred in England, and I have seen specimens which had tail coverts (and also hackles) longer than those of any European breed, but so far as I know no specimens bred in Europe have produced the extraordinary length of feather that is known to occur in Japanese specimens, for example in the two stuffed specimens in the hall of the Natural History Museum. It seems to me reasonable to conclude that the hereditary effect is due to the artificial irritation applied to a long succession of generations.

Penzance, June 5. J. T. CUNNINGHAM.

Variation in a Bee.

ON September 24, some years ago, I collected at Mesilla, New Mexico, four examples of a wild bee of the genus Epeolus, the species being probably identical with *Epeolus bardus* of Cresson. In every one of these specimens the second transverso-cubital nervure is incomplete, its lower half being wanting, on one or both sides. In one example only is the nervure incomplete on both sides; in the other three it is incomplete on the right side only. Such aberrations are not very uncommon among bees, but they usually occur in single examples, and this is the best instance known to me of their being inherited by a number of individuals. What is here clearly a sport seems in a fair way to become a racial character, and we seem to have a good example of Bateson's "discontinuous variation." In the genus Halictus certain species have only two submarginal cells, instead of the usual three, and the same is true of Andrena. These peculiar species are related to different groups of the genera to which they belong, so that if it is proposed to regard them as pertaining to distinct subgenera (or genera) by reason of their venation, it becomes necessary to propose several sub-generic names instead of one, because of the independent evolution of the species. That this evolution has resulted from the perpetuation of sports such as that described above we can hardly doubt, but we are not thereby compelled to admit that it may not also be beneficial to the species.

T. D. A. COCKERELL.

East Las Vegas, New Mexico, U.S.A., May 25.

Foreign Oysters Acquiring Characters of Natives.

THE facts contained in Mr. Tabor's letter, however interesting, supply no evidence for or against Lamarckism. When at Whitstable, the individual French oyster has certain characters impressed upon it by its environment. The next generation, when compared with the natives, show certain peculiarities, such as greater thickness of shell and greater growing power. But this also we are able to interpret as the reponse of the individual to the environment. If the peculiarities appear in many successive generations, the same explanation will account for the facts. If, however, Lamarckians could show that the effect of the environment, as the generations succeed one another, is cumulative, that the characters in question become *progressively* accentuated, then they would prove their case. But it does not appear that they have any such evidence at their command. F. W. HEADLEY.

Haileybury College, Hertford.

ITALIAN EXPLORATION IN ARCTIC REGIONS.

THE recent success of the Duke of the Abruzzi's expedition, which carried the Italian flag nearer the North Pole than ever flag flew before, has doubtless prepared a public in Italy for the literature of Polar exploration. The firm of Hoepli, who have conferred many favours on Italian-speaking geographers, have just published a history of Polar exploration in the nineteenth century by Signor Hugues.[1] The book makes no claim to originality, being merely a condensed popular description of the Polar voyages of the late century, and although more detailed on account of the shorter range of time dealt with, and coming down to the year 1900, it cannot compare with General Greely's compact handbook as a work of reference for the student. The most serious drawback is the want of a bibliography or a uniform

[1] "Luigi Hugues—Le Esplorazioni Polari nel Secolo XIX." Pp. xx + 374. Maps and Illustrations. (Milano: Ulrici Hoepli, 1901). Price 12 lire.

system of acknowledging sources of information. Another, which strikes an English reader, is the curiously unfamiliar aspect of well-known names of people and places in their Italian form—Giovanni Ross and Giuseppe Wiggins, Terra del Re Guglielmo and San Giovanni de Terranuova require some thinking over. Where so many personal names are foreign to the author, misprints may easily escape detection, and in the index a cursory inspection reveals about a dozen slips, of which the worst are Gordfelow for Goodfellow, and Newes for Newnes. Probably no English author could handle more than 600 foreign names with fewer accidents. Except for a tangle of dates on p. 98, and the necessary baldness in the treatment of some picturesque episodes induced by brevity, the narrative is clear, interesting and, so far as we can test it, correct. Most space is, of course, given to the Arctic regions; but the history of South Polar voyages is also summarised.

The members of the Italian Arctic expedition had a magnificent reception in Rome on January 14, the description of which, together with the addresses of the Duke of the Abruzzi and Captain Cagni, occupy practically the whole February number of the *Bollettino* of the Italian Geographical Society. The hall was splendidly decorated with flags and Arctic trophies; and the King and Queen of Italy, with other members of the Royal family, the great officers of State and the Diplomatic Corps, as well as the heads of the scientific bodies in Rome, were present.

The Duke of the Abruzzi described the equipment of the expedition and the voyage of the famous whaler *Jason*, renamed the *Stella Polare*, to her winter quarters in Teplitz Bay; and after Captain Cagni had told the story of his great sledge journey over the ice, H.R.H. resumed the narrative of his sojourn at the base and the return of the party to Europe.

The following is a brief summary of the facts:—

The *Stella Polare* left Archangel, where she had called for dogs, on July 12, 1899, and, after some delay in the ice, passed Cape Flora, in Franz Josef Land, on the 26th, sailed up the strait named by Jackson the British Channel and along the shore of the Queen Victoria Sea to a point in 82° 4′, just north of Cape Fligely on Prince Rudolf Island, which was reached on August 8th, after a good deal of trouble from the ice. Teplitz Bay, in 81° 47′, was chosen for wintering the ship; the dogs and stores were landed there, and the ship, having been damaged on September 9 by an ice-pressure, the party was obliged to land and live on shore. During the winter the Duke of the Abruzzi was severely frost-bitten in the hand and was obliged to abandon his intention of accompanying the sledge expedition to the north in spring. The command of this expedition accordingly devolved on Captain Cagni. The sledges used were of Nansen's pattern; the sleeping bags for the men were made of reindeer skin; pemmican was the chief food relied on, and petroleum was used for cooking. The expedition was marshalled in three divisions, each consisting of three men and four sledges, on which were placed 180 rations for men and 1150 for dogs. The provisions of the first division would suffice for the whole party for fifteen days after leaving the island, when the people of that division would return. The provisions of the second division would supply the two remaining groups for fifteen days more and then suffice to allow its members to return to the base, while the third group with their intact store of provisions should be able to push on for fifteen days more, or forty-five days from the base, before requiring to return.

In some preliminary sledge trips in February a temperature descending to −52° C. (−62° F.) was recorded, this being the lower limit of the graduation of the minimum thermometer. The start for the real attempt on the pole was made on March 11, 1900, when the caravan of three parties struck out boldly across the sea-ice, bound north, Captain Evenson and two sailors accompanying the expedition for two days with a thirteenth sledge. The advance at first was slow, on account of bad weather and rough ice. On March 22 the first division, consisting of Lieut. Quirini, the guide Ollier, and the engineer Stökken, left to return to Teplitz Bay. This party has never since been heard of, and there is little doubt that all three have perished. On March 31 the second group went back, and Captain Cagni continued on his way with three companions, Italian Alpine guides named Petigax, Fenouillet and Canepa. By sending back the other parties some days earlier than was at first intended he was able to retain a larger supply of provisions. Six sledges were taken on, and in spite of the difficulties of the way the party made excellent progress, and by reducing the rations they were able to continue the northward march to 86° 33′ 49″ in 64° 30′ E., which was attained on April 25. The journey at times was comparatively easy, the ice in places being smooth and covered with firm snow, but frequent pressure-ridges had to be surmounted and proved serious obstacles. In the latter part of the journey, when the temperature did not descend below zero Fahrenheit, lanes of water often opened with dangerous suddenness and caused great delay, while frequent gales and bad weather of every kind were encountered. The return journey, with rapidly dwindling provisions and diminished strength, was extremely laborious and the steady drift of the ice to the westward was a very serious difficulty, and, despite an increasing easterly component in the direction of march, the first land sighted was Harley and Neale Islands and Cape Mill, fifty miles west by south of Teplitz Bay. It was June 22 before the base was reached, and Captain Cagni had been absent 104 days, making (apart from the loss of the three men in the missing party) perhaps the most successful sledge journey ever accomplished in the Arctic regions, and certainly reaching the highest latitude.

The *Stella Polare*, after temporary repairs, was released from the ice with great difficulty, and only succeeded in getting away from Teplitz Bay on August 15, after which a good passage was made to Norway.

The results of the expedition are touched on slightly. Petermann Land and King Oscar Land, reported by Payer, have been shown not to exist in sight of the positions assigned to them. Cape Fligely (81° 51′) is proved to be the most northerly point of Franz Josef Land, and Cape Sherard Osborn does not belong to the same island, if it has any real existence. Doubt is thrown on the existence of the islands reported by Wellman north of Hvidtenland; but the maps of the Jackson expedition appear to have been found accurate so far as they could be tested. A year's meteorological and magnetic observations were obtained at Teplitz Bay, and gravity and tidal observations were also carried out. Prince Rudolf Island was found to consist entirely of basaltic rock. Animal life was not found very abundant, polar bears being the only common land mammals, and no new birds appear to have been discovered.

HAILSTORM ARTILLERY.

IN the absence of any recognised English equivalent for the expressive German term *Das Wetterschiessen*, I have thought it best in the heading of this article to avoid a literal translation of it lest it should give rise to misunderstanding. "Weather shooting" does not refer to any haphazard or empirical attempts to foretell the weather, but to a practice which has lately come to have great vogue in Styria, Italy and elsewhere of firing off charges of gunpowder to protect the vineyards against injury from hail. So popular indeed has the practice become in some districts that there is danger of the

cost of the protection exceeding that of any damage likely to be caused by the hail.

The idea that the weather can be affected by the discharge of gunpowder is not a new one. There have been various traditions of rain falling after, and presumably in consequence of, the cannonade of a battle, and I have some recollection of an account in English newspapers of an American enterprise for terminating a drought by a sufficiency of gunpowder.

Weather shooting as now practised has, however, a more definite purpose than merely causing rain. Its object is to prevent the downpour of hail by shooting when thunder or hail clouds threaten. Even this form of the application of gunpowder to the management of the weather is by no means new. The *Meteorologische Zeitschrift* of March 1900 states, on the authority of Arago, that in the seventeenth century a fleet, anchored off Cartagena (South America), dispersed a daily afternoon thunderstorm by a daily bombardment; and Leonardo da Vinci is said to have asserted that damage by hail could be averted by mounting mortars on the hills from which the storm-clouds came and shooting at them. Quite early in the past century the matter was taken up in the neighbourhood of Macon. The recent development, which has spread very widely, is most conspicuously represented by the arrangements of Bürgermeister Stiger, of Windisch-Feistritz, in Styria, where they were originally introduced in 1896 in the form of a vine-dressers' volunteer artillery. Batteries of ten heavy mortars to take a charge of 120 grammes of powder, served by six men each, were placed at twelve separate stations within two square kilometres at a high level near Windisch-Feistritz. As soon as a downpour of hail threatened, the 120 mortars were fired "incessantly" until the danger was past. The second year thirty-three stations were at work, and the third fifty-six. It is reported that this energetic proceeding has completely protected the region from hail and has mitigated the damage from lightning; and as Bürgermeister Stiger apparently introduced the system as an alternative to covering the district with wire of close mesh, the damage must have been previously regarded as a serious matter. Other places have been less successful, and the Austrian Government and local authorities have taken steps to inquire into the effectiveness of the shooting. But the vineyard districts are not willing to wait for the report of the inquiry; they are satisfied that they only failed because they did not shoot early enough or often enough, and only desire to shoot more and oftener.

It is not quite clear how the effects of the shooting are manifested. In some cases it would appear that the shooting dispersed the clouds altogether, in others that it caused rain, sometimes heavy rain, sometimes a genial and welcome rainfall instead of the malignant pelt of the hail.

Dr. Pernter, of the Austrian Meteorological Department, was of the commission appointed to inquire into the matter, and in the September number of the *Meteorologische Zeitschrift* he gives a most interesting account of some experiments in connection with the inquiry. From that account it appears that there are three forms of apparatus employed, differing in size. A small cylindrical mortar with a large conical mouthpiece is the general form of the apparatus.

The conical portion of the smallest system (System Unger) is 2 metres long, that of the longest (System Suschnig) is 4 metres long; the former takes a charge of powder up to about 60 grammes, the latter up to about 250 grammes. Briefly, the latter is the most effective implement, and a charge of 180 grammes is the best suited for the purpose.

The effect of the shot is to produce, besides noise, a vortex ring of most impressive dimensions and energy. It would start with a loud hum and settle down to a whistle. When the gunpowder charge was most suitable, it would tear a thick paper screen to pieces at 100 metres distance from the mortar and pull the wooden framework of the screen apart and hurl the pieces about. Dr. Pernter, indeed, becomes quite eloquent in his description of the behaviour of these rings as astonishing physical experiments quite apart from any practical interest they may have as affecting the weather.

The position of the ring is recognisable by its whistle after it has become invisible, and its duration is estimated by the duration of the whistling. In the firing the rings are shot upwards, and it is assumed that the effect of the shooting depends upon them.

Dr. Pernter's experiments were directed mainly towards ascertaining the velocity and the length of the path of the rings, with the ultimate object of determining whether they could reach the levels of the lowest stratum of rain cloud. Determining the velocities from a very large number of experiments with charges of different weight, he obtained in the most favourable circumstances with the Suschnig apparatus an initial velocity of about 55 metres a second and a height of 400 metres as the extreme probable limit of the best shots. Thus the experiments seem to show that the rings would not reach the storm-clouds at the 1000-metre level, but as the local people were convinced from their own observations that the storm-clouds in the neighbourhood of St. Katherein (where the experiments took place) were to be found at 800 metres, and as the shooting-gear was fixed at elevations of some 500 metres, it seemed possible that the rings might just reach the clouds.

Such is the result of the investigation, with the addition that the smaller apparatus would not carry nearly so far, nor would the rings have anything like so much energy as those from the larger apparatus, whence it follows that if we wish to shoot the clouds effectively we must use the largest-sized mortars, taking 180 grammes of powder, and we should then be a little uncertain whether the ring would travel far enough.

Various theories have, of course, been suggested to account for the protection from hail alleged to be secured by this shooting. Supersaturated air from which the rain is liberated, a labile state of atmospheric equilibrium disturbed by the discharge, globules of overcooled water, still liquid below the freezing-point, which would form large hail-drops if they were allowed to coalesce but are solidified separately by the shock, and many other suggestions have been put forward as the state of things precedent to the hail shower, which is disarranged by the shooting. There seems, indeed, to be a disposition to see what curious conditions our present knowledge of the physics of the atmosphere can account for, and then wonder whether one of them might be the condition of things in a thunder cloud. Theory is very much at a disadvantage, because it is not at all clear what has to be explained, and it is, indeed, difficult to account for facts when we do not know what are the facts to be accounted for.

Dr. Hann has suggested, very properly, that the effect of shooting upon a winter fog should be ascertained. There appears to be some evidence that gun-firing clears the air of such a fog. But whether theory is to regard the noise or the smoke or the energy of the vortex ring as the cause of the effect, or whether, indeed, there is any effect to be explained, is not yet finally established. At the same time, no one is prepared to say that no effect is possible or is willing to lay claim to sufficient knowledge of the conditions of the atmosphere immediately preceding a hailstorm to venture any categorical opinion on the various theories. The subject was brought before the conference of meteorologists last year at Paris by several writers, and some additional information about it will doubtless appear in the report of that conference when it is published; in the meantime,

the prominent meteorologists of the countries where there is a great popular demand for weather shooting, Styria, Hungary, Italy, Switzerland and France, are unanimous in the desire that the demand may lead to definite investigation of the nature of the processes taking place in thunderstorms, and especially in the formation of hail, which will lead to a real advance in our knowledge of these phenomena and will furnish a satisfactory basis for a theory of weather shooting. W. N. SHAW.

VIRIAMU JONES.

YET another gap in the front rank of science. But yesterday it was Fitzgerald, then Rowland, and now—Viriamu Jones is dead, the last, like the first, especially great in inspiring others.

Son of a working collier, a collier with rare gifts, the "poet-preacher" of Wales who thrilled with his silver tongue the gathered thousands and moved the multitude with his mighty eloquence, Viriamu inherited all those qualities which tend to greatness and came into daily contact with them in his own home. His very name indicated what was expected of him, for "Viriamu" was the name of the martyr missionary Williams, rendered as best it could be by the Polynesian tongue.

At the earliest permissible age of sixteen, Jones passed the London matriculation examination and won the scholarship in geology, the subject in which he took his degree with first-class honours three years later. Meanwhile he was gaining prizes, medals and scholarships at University College, London, and was elected Brackenbury scholar at Balliol College, Oxford. Going there at the age of twenty, he came under the direct influence of Jowett and commenced that personal friendship which influenced his whole life. After obtaining a first class in mathematical moderations in the final school of mathematics and in the final natural science school, he was elected principal and professor of mathematics and physics in the Firth College, Sheffield, when only twenty-five.

How he used to laugh because he always knew exactly what an examiner wanted; and what a true estimate did he form of the poverty of the examination system to test a man's real powers. How sympathetic was he when one was despondent at the unpractical character of the "intellectual miser," the student who spends his time acquiring and hoarding knowledge without giving the world a single new idea of his own. His views on education were of the broadest; to him the study of Greek and Latin, a problem in mathematics, the adjustment of a Whitworth measuring machine, were all equally living, and in the niceties of all three he showed the same absorbing interest.

No wonder, then, that when the first principal of the University College of South Wales had to be appointed the council chose the youngest of the thirty applicants —for that youngest was Jones in his twenty-seventh year. And no appointment ever made was better justified. The many speeches to his memory, the letters that have flowed in from every side, including one from the King, all prove how the work of the principal was appreciated, how the man was idolised.

He placed the University College system of Wales on a truly educational and democratic basis, and shaped the educational policy of his country by formulating the system of secondary education, which fills the gap between the primary schools and the colleges, and by the part he played in establishing a University for Wales.

The charm of his personality, his magic smile, the grace of his diction and his winning persuasiveness secured success where others could but court failure. Some 70,000*l*. he gathered together for the building of the new college, but a free site was still wanting, for this had been refused by the Corporation last summer, when Jones was too ill to be in England. Returning, however, in the autumn, he sought an interview, as a belated member of the deputation, charmed the Corporation into reversing their decision, and won for his college a site as a free gift. Well might Sir William Harcourt, when Chancellor of the Exchequer, jocularly say that Principal Jones was the cleverest beggar he had ever met with, and about the only one he could not get rid of without promising to give what was asked for.

Deep is the gratitude the college feels for its first principal; sincere is the praise Wales is reverently showering on the young first Vice-Chancellor of its University.

During the last few years I saw much of Viriamu Jones in connection with the construction of electrical standards, and I was always struck with surprise at the way in which one who found his greatest relaxation in studying the poetry of the most regular and attentive of his father's congregation—Robert Browning —discussed in detail why he thought the physical theories of the day too fanciful, and criticised the modern electrical measuring instruments for not being constructed on engineering lines.

His immediate ambition was to provide the National Physical Laboratory with electrical standards constructed like well-designed engineering machine tools rather than the ordinary physical laboratory apparatus, and it was towards such an end that his scientific work of recent years tended. A certain City company had promised him the funds for a far more perfect Lorenz apparatus than any yet made, and many were our talks about its details, how the coil and rotating disc were to be horizontal, and the non-magnetic driver a turbine, &c.

His first paper on this particular subject of electrical standards was published in 1888, and consisted of a determination of the coefficient of mutual induction of a circle and coaxial helix in connection with constructing the coil of a Lorenz apparatus by winding a *single* layer of wire in a screw thread cut on the surface of a large brass cylinder. For it seemed probable that with such a coil Lord Rayleigh's formula, which is a first approximation, would not give a result of sufficient accuracy, and Jones succeeded by a method of direct integration in obtaining a comparatively simple formula which gave the coefficient of mutual induction with greater accuracy, and enabled a single larger coil, the geometry of which can be better known than one of many convolutions, to be employed. In the following year he discussed the employment of Lissajous' figures for determining the rate of rotation of the disc of the Lorenz apparatus and of a Morse receiver for measuring the periodic time of the tuning-fork employed.

In 1890 he announced, at the meeting of the British Association at Leeds, that with the use of his specially constructed Lorenz apparatus the ohm was equal to the resistance of 106·307 centimetres of mercury one square millimetre in cross-section at 0° C., the complete account of the apparatus, its use, the mathematical calculations employed and the results obtained being published in the *Philosophical Transactions* of the Royal Society for 1891. Three years later he was elected a Fellow of that Society.

Appendix iii. of the 1893 Report of the British Association Committee on electrical standards consists of the results of his use of the Lorenz apparatus to measure directly the value of commercial low resistances of the order of 1/5000th of an ohm with an accuracy of one part in 12,000, as contrasted with the comparison of such resistances with a known standard in the ordinary ways. Appendix ii. of the 1894 Report deals with a determination of the ohm by measuring the absolute value of the resistances of a combination of four coils which had been compared with the standards of resistance in the Cavendish Laboratory; while in Appendix ii.

of the 1897 Report is a full description of the use of a Lorenz apparatus (constructed to Jones's designs for the McGill University) in determining the absolute values of the same four coils. From this it followed that a Board of Trade ohm equalled 1·00026 true ohms.

In the interim—viz. in 1896—he gave an account of the correction that would have to be made in consequence of a very slight ellipticity of his large brass coil, which he found to exist in 1894, and he showed that his 1900 value of 106·307 centimetres for the ohm would have to be increased to 106·319 on this account.

The formula developed by him in 1888 for the calculation of the mutual induction of a circle and a coaxial helix, although comparatively simple, in view of the accuracy obtained with its use, led, in reality, to a long laborious calculation when employed in practice. Consequently he spent some of the leisure of his voyage home from Montreal in 1897 in working out a simplification of the method previously described and a more general solution. And the account of this formed the substance of the paper he read before the Royal Society in November of that year.

Jones' ampere balance, briefly described in Appendix iii. of the 1898 British Association Report, was designed of a form which would readily lend itself to the use of a new formula (also developed in the preceding Royal Society paper) for the force between a uniform cylindrical current sheet and a coaxial helix, which could be readily expressed in elliptic integrals.

The liberality of the British Association, and of Sir Andrew Noble, will enable his standard ampere balance to be realised. The love of his friend will accomplish its completion.

Through Cardiff's hushed streets, contrasting strangely with their noisy traffic of other Saturdays, the long procession, last week, wended its way. Bright was the van with the mounted escort and the firemen's glittering helmets, sombre the rear with the girl students in their caps and gowns. By his father's side, high up on the hill overlooking Swansea Bay, we laid him—the man of high ideals, the man who had lived a long long life though dead at the age of forty-five. W. E. AYRTON.

It was right that one who did so much for the educational advancement of Wales should be given a public funeral. A memorial service was held at the Park Hall, Cardiff, on Saturday morning; the Bishop of Llandaff read the lessons, while the sermon was preached by the Rev. J. Williamson. After the service there was a procession to the Great Western Railway Station, and the gathering included representatives of important municipal and public bodies, the University of Wales, the University colleges of Wales and other educational authorities and institutions. A special train conveyed the body and the mourners to Swansea, where the interment took place, the Mayor of Swansea and the members of the Corporation, as well as representatives of local educational bodies, taking part in the mournful ceremony.

NOTES.

THE Royal Society announces that it is about to make the first award of the Mackinnon research studentship. The studentship is founded under a bequest to the Royal Society by the late Sir William Mackinnon, Director-General of the Medical Department of the Army, of the residue of his estate upon trust to be applied for the foundation and endowment of such prizes or scholarships for the special purpose of furthering natural and physical science, including geology and astronomy, and of furthering original research and investigation in pathology as the Society may think best and most conducive to the promotion of those sciences and of original discoveries therein. The committee appointed by the council of the Royal Society to advise upon the best mode of giving effect to the intentions of the testator recommended that the award should be in the nature of a studentship for the encouragement of research rather than a prize for the reward of past achievements, and that the studentship should be devoted to the maintenance of a student engaged in such researches as were indicated by the testator. The studentship will be awarded this year in one of the biological sciences, including physiology and anatomy, pathology, botany, palæontology and zoology; it will be awarded for one year, but will be renewable for a second year. The studentship is at present of the annual value of 150*l.*, but the awards may be multiplied in future, upon the determination of certain outstanding charges upon the property. Applications must be received not later than June 26 by the assistant secretary of the Royal Society, from whom further particulars may be obtained.

THE gold medal presented biennially by the Pharmaceutical Society in memory of Daniel Hanbury, for high excellence in the prosecution or promotion of research in connection with the chemistry and natural history of drugs, has this year been awarded to Dr. George Watt, reporter on economic products to the Government of India. Dr. Watt, says the *Pharmaceutical Journal*, was born at Old Meldrum, Aberdeenshire, on April 24, 1851, and was educated at the Grammar School, King's College, and Marischal College, Aberdeen, subsequently graduating as M.B., with first-class honours, in the University of Glasgow. He became assistant professor of botany at Aberdeen in 1871, and professor of botany at Calcutta University in 1873. His best-known work is the "Dictionary of the Economic Products of India," but he is also editor of the *Agricultural Ledger*, and of the report of the Central Indigenous Drugs Committee of India for 1900, as well as the author of reports on the pests and blights of the tea plant, on rhea and China grass, on lac and the lac industries of India, and on a plague in the betti-nut palms of India. He has also published a "Flora of Chamba," a monograph on the Primulaceæ, and other scientific and technical works. Dr. Watt is still engaged in clearing up the difficulties that surround the botanical sources of aconite roots of Indian commerce, and has only recently furnished material for the investigation of kino.

THE conditions which will control the administration of Mr. Carnegie's munificent gift to Scottish Universities have now been published, and they remove the difficulties which presented themselves when the announcement of the donation was made, but no particulars were available as to its allocation. The annual income from the trust is estimated at 104,000*l.*, and it is to be administered by an executive committee of nine members, the first committee being constituted as follows:—The Earl of Elgin, who is to act as chairman, Lord Balfour of Burleigh, Lord Kinnear, Sir Henry E. Roscoe, Mr. Shaw, the Lord Provost of Edinburgh, the Lord Provost of Glasgow. Two remaining members are to be two of four trustees nominated by the University Courts, the members for Edinburgh and Aberdeen acting during the first two years, and the members for Glasgow and St. Andrews acting during the second two years. One-half of the net annual income is to be applied towards the improvement and expansion of the Universities of Scotland in the faculties of science and medicine, also for improving and extending the opportunities for scientific study and research, and for increasing the facilities for acquiring a knowledge of history, economics, English literature and modern languages, and such other subjects cognate to a technical or commercial education as can be brought within the scope of the University curriculum. The other half of the income, or such part thereof as in each year may be found requisite, is to be devoted to the payment of the whole or part of the ordinary class fees exigible by the Universities from students of Scottish

birth or extraction and of sixteen years of age and upwards, or scholars who have given two years' attendance after the age of fourteen years at State-aided schools in Scotland, or at such other schools and institutions in Scotland as are under the inspection of the Scotch Education Department. Any surplus remaining in any year from the income applicable to this head of expenditure is to be applied to the first head of expenditure. In the case of schools or institutions in Scotland established to provide technical or commercial education, the committee may recognise classes which, though outside the present range of the University curriculum, can be accepted as doing work of a University level, and may allow them and the students thereof to participate under both heads of the trust deed. The benefit of the trust is to be available to students of both sexes. The trustees are to have full power, by a majority of two-thirds of their number, to modify the conditions under which the funds may be applied so as to secure that these shall always be applied in the manner best adapted to meet the purposes of the donor as is expressed in the constitution, according to the changed conditions of the time.

THE new Pathological Institute at the London Hospital will be formally opened by Sir Henry Roscoe, vice-chancellor of the University of London, on Wednesday, July 10, at 3 o'clock.

THE American Chemical Society has elected the following honorary members:—Prof. W. Ramsay, Sir Henry E. Roscoe, Prof. E. Fischer, Berlin, Prof. A. Baeyer, Munich, and Prof. G. Lunge, Zurich.

THE gentlemen selected by the council of the Royal Society for admission as Fellows this year were elected at the meeting held last week. The qualifications of the new Fellows were given in NATURE of May 9 (p. 36).

THE sixty-ninth annual meeting of the British Medical Association will be held at Cheltenham on July 30—August 2. The president-elect is Dr. G. B. Ferguson. An address in medicine will be delivered by Dr. J. F. Goodhart and an address in surgery by Sir William Thomson. The scientific business of the meeting will be conducted in thirteen sections.

THE establishment of a Ministry of Commerce, under a minister of business experience, is being actively urged by the *Daily Express*. A provisional committee has been formed, consisting of a large number of Members of Parliament, civic authorities, presidents of Chambers of Commerce, and heads of important business firms. It is proposed to hold a public meeting in London at an early date, with the object of forming an association and generally to take practical steps in the organisation of the movement.

THE Paris correspondent of the *Times* announces that M. Th. Ribot, professor of experimental psychology at the Collège de France, the founder of the *Revue Philosophique* and the inspirer of an entire generation of students and professor of the new psychology, not only in France but all over the world, will retire on a pension, at his own request, at the beginning of November.

WE are informed that, in the unavoidable absence of the president of the Institution of Electrical Engineers, Mr. Alexander Siemens (past president) will, at the unanimous request of the council, assume the leadership of the Institution party throughout the visit to Germany. Members are reminded that their applications for tickets should be forwarded before Saturday next, June 15.

ACCORDING to the latest returns the population of Paris increased during the last five years by 6·98 per cent. At the last census, which was taken at the end of March 1896, the inhabitants numbered 2,536,834; but at the present time the total is 2,714,068.

AT the annual meeting of the Akademie der Wissenschaften, of Vienna, on June 1, it was announced that Prof. Eduard Suess had been unanimously re-elected president for a further period of three years. Thereupon the Professor delivered his presidential address, which contained, amongst other scientific statements, some valuable references concerning the life and works of the late Prof. Max Müller, of Oxford, who for many years was honorary member of the Akademie. Prof. Berthelot, of Paris, was nominated honorary member, and the following gentlemen were elected as foreign corresponding members:—Profs. Schlegel (Leyden), Oppert (Paris), Linde (Munich), Retzius (Stockholm), Kowalesky (St. Petersburg).

BY the courtesy of the editor of the *Chemist and Druggist* we are able to give an illustration of the monument to Pasteur, shortly to be erected at Dôle, where he was born. The statue,

Monument to Pasteur, to be erected at Dôle.

the sculptor of which is M. Antonin Carles, is in bronze, and stands on a pedestal eight metres high. The figures at the base of the monument represent Humanity bringing two children to Pasteur, while Science offers him a palm.

THE *Electrician* announces that the system of etheric signalling devised by Sir William Preece has been successfully installed for the purpose of placing Rathlin Island in telegraphic communication with Ballycastle. The distance over which the signals are transmitted is about ten miles, as the waves go, and the lengths of inductive wire employed on each side are one and six miles respectively, the shorter length being on the island. The telephone was used as the receiver, with Morse signals transmitted by means of a "buzzer"—a more rapid if less sensitive arrangement than the Marconi coherer.

REFERRING to the death of the ethnologist Dr. Arthur Hazelius, on May 27, in his sixty-eighth year, the *Athenaeum* says he was the founder of the Ethnographical Nordische Museum and of the unique and interesting Skansen, the open-air museum in the Zoological Garden of Stockholm, the result of nearly thirty years of labour, where the national life of old

Sweden is represented in vivid fashion, not merely by means of buildings, but also by the festivals and music of earlier times. Dr. Hazelius's son has, it is stated, been elected to succeed him as director of the Nordische Museum.

THE death is announced of Mr. William Walton at Little Shelford, near Cambridge. He was born in 1813 and graduated as a member of Trinity College in the mathematical tripos of 1836, being eighth wrangler. After taking his degree he remained at Cambridge and became a successful private tutor and lecturer in mathematics. He published a considerable number of mathematical treatises which for many years were used as text-books by students. His chief works were a treatise in illustration of the principles of theoretical mechanics and a volume on the differential calculus.

AN International Fire Prevention Congress met at Berlin last week, under the presidency of Count Komarowsky. The first resolution, which was unanimously carried, was proposed by Mr. Edwin O. Sachs, and was in the following terms: (1) That the serious investigation of the fire resistance of materials and systems of construction should be supported both by the Government and local authorities, as well as by those technical societies to whose members the results of such investigations are important in the practice of their professions. (2) In view of the fact that identical materials and systems of construction are frequently employed in different countries, an effort should be made to standardise the results obtained from fire tests in such a manner that the investigations made in different countries should be compared in a practical manner with due regard to units of measurement and temperature.

WE regret to see the announcement in the *Times* of the death of Prof. Bleicher, director of the school of pharmacy in the University of Nancy, and formerly professor of natural history at the same school. He was shot by a pharmacist from whom a sample of cinchona had been seized for analysis at the school. This crime has deprived France of one of the scholars who have done most to reveal to the world the geological interest of the frontier provinces of France. Prof. Bleicher's "Les Vosges, Le Sol, et Ses Habitants" is a classical treatise which every traveller in Alsace-Lorraine should always carry with him. Every year Prof. Bleicher spent his holidays on one or other of the slopes of the Vosges, studying the stratifications, the rocks, the glacial marks, all the features, in a word, of this interesting region, upon which he had published a large number of memoirs. He had begun life as Médecin-Major in the French African army, but left his work there in 1877 to become professor at Nancy, where he was very popular, often conducting students' scientific expeditions.

THE scientific study of plant associations and conditions of growth of crops was urged by Mr. R. Hedger Wallace in a lecture delivered at the museum of the Royal Botanic Society on Friday last, Sir George Kekewich, K.C.B., being in the chair. He remarked that commercial crop cultivation as a subject correlated the practical details taught by economic geography and botany. The mapping of plant associations would be of service, because wherever a man wishes to cultivate the ground a study of its actual flora is the most trustworthy guide to the possibilities of success or failure of new species. To the agriculturist and horticulturist the characteristics of plant areas are better guides than those of climate alone, because in plant distribution the influence of soil and drainage is correlated with that of climate. What are needed, the lecturer stated, are maps showing natural plant areas, cultivated crop areas and zones of cultivation, distinguished by definite colours like a geological map. With respect to plant distribution and zones of cultivation, attention was directed to the work that has been done in Germany, especially by Profs. Oscar Drude and Engelbrecht. The botanist who studies the distribution of plants usually eliminates all consideration of the plants that are cultivated by man as vitiating his inquiry. Engelbrecht, on the other hand, deals entirely with the distribution of cultivated plants, though his survey is restricted to agricultural and horticultural produce grown outside the tropics. To study the commercial crop cultivation of a country the geographical conditions should be noted. Land forms, that is, the relief of the land, have a powerful influence, indirectly as well as directly, on plants, animals and human beings. An endeavour should therefore be made to gain some idea of what might be termed the climatic control of land forms, and the influence of land forms on natural flora and cultivated crops.

WITH reference to the inquiry of a correspondent as to the appearance of the Hoopoe on Lundy Island (p. 132), Mr. W. H. Graham writes from Fowey, Cornwall, "I dare say your correspondent would be interested to know that I saw a Hoopoe here in 1900, and one has been seen here this year; both were seen in the early spring, March, I think. Possibly those on Lundy Island have crossed from Cornwall."

THE invention of the Poulsen telegraphone, a full description of which in its latest form we hope shortly to publish, seems to have stimulated efforts to replace the wax cylinder phonograph by some more satisfactory arrangement. Descriptions of two new phonographs have quite recently been published—one the invention of Prof. Nernst and R. von Lieben, and the other invented by E. Ruhmer. A full account of Prof. Nernst's arrangement appears in the *Electrician* for June 7. The principle of which he makes use is the alteration of polarisation capacity and surface resistance of a metal used as an electrode in an electrolytic bath. A copper disc about 3 mm. thick is rotated at a fairly high speed, whilst there presses against its edge a thin wedge of wood soaked in an electrolyte. The secondary currents from the induction coil of a microphone transmitter are caused to pass through this contact and leave a record on the edge of the disc on account of the varying amount of chemical change produced. A telephone receiver is then substituted for the microphone, a battery being included in the circuit, and on again rotating the disc a reproduction of the sound is obtained. The best results seem to have been given by a solution of potassium zincate, using the edge of the copper disc as kathode, the wedge standing in a bath of the solution into which a zinc anode dips. With this, it is stated, the sounds can be reproduced clearly and distinctly two or three hundred times. The record can be cleaned off with fine emery paper.

RUHMER'S phonograph is based on an entirely different principle, thus making the third new phonographic method worked out in the past few months. The information at present at hand is, however, very scanty, so that we cannot do more than state the general claims of the inventor. Herr Ruhmer photographs, on a moving film, a sensitive flame which is being affected by sound vibrations, and thus obtains on the film a band of varying intensity; light is then projected through this band on to a selenium cell which is included in circuit with a battery and telephone. The variations in intensity as the film is passed before the source of light cause variations in current in the telephone circuit which reproduce the original sounds. The reproduction, it is said, is clearer than in the Poulsen telegraphone, and as an additional advantage multiplication of the records can be carried out photographically to any desired extent.

DURING the last few years the Danish Meteorological Institute has issued a very useful volume entitled "Nautical-Meteorological Annual." That for the year 1900 has just appeared and contains a summary of the state of the ice in the Arctic seas for

each of the months March to August, with maps. Generally speaking, there were considerable masses of ice during the season 1900 in the north-west of Barents Sea, around Spitzbergen and in the Kara Sea, less than usual between Franz Joseph Land and Nova Zembla and on the east coast of Greenland, while in Baffin Bay and near Labrador the conditions were particularly favourable. The volume also contains tables showing the diurnal amplitude of the air at the various Danish light-vessels, and the surface temperature of the sea in the northern Atlantic Ocean and Davis Strait. The greater part of the work is taken up by carefully compiled tables of general meteorological observations, taken every four hours by the light-keepers, together with monthly means. These form a valuable contribution to the meteorological statistics of the northern parts of Europe.

M. D. KORDA announces in the *Bulletin* of the French Physical Society that in a fraction of a minute he has succeeded in crystallising ferrosilicium in the bottom of a crucible by cooling with water. The form of the crystals varies with the proportion of silicon—long needles for 10 to 100 of silicon (Fe_2Si), tetrahedra of 1 to 10 mm. length of side for 22 to 23 per cent. of silicon (FeSi), and laminæ of micaceous character for 50 per cent. of silicon ($FeSi_2$). Crystals of ferromanganese or ferrochromium can be similarly formed.

DR. EMILIO ODDONE describes, in the *Rendiconto* of the Lombardy Institution, experiments conducted for the purpose of determining the mean coefficient of transparency of the air over distances considerably greater than those previously experimented on, and he gives examples of the application of this method to distances of 45, 85 and 135 kilometres. The coefficients are fairly high, increasing with the distance, and the ultimate values are only slightly less than those corresponding to vertical vision. From this property, Dr. Oddone thinks it possible to calculate approximately the thickness of the atmosphere in the direction of the zenith.

THE *Archives* of the Röntgen Ray contains a short programme of the Röntgen Exhibition to be held in Hamburg in connection with the seventy-third meeting of the Deutscher Naturforscher und Aerzte. The scientific part will be in the hands of Dr. Albers-Schönberg, Dr. Walter and Dr. Hahn, while the literary part will be taken by Messrs. Lucas Gräfe and Sillem. The physical section will include induction coils and contact breakers, portable apparatus, tubes, fluorescent screens, operating tables, stereoscopes and other accessories, power for working the coils, &c., being obtainable at 220 volts continuous and 120 volts alternating current. The medical section will exhibit the latest achievements in radiography and the therapeutic uses of Röntgen rays. In addition with the above it is mentioned that at a recent sitting of the Prussian Kultus Ministerium the Universities received a grant of 1000*l.* for additions to the Röntgen ray departments.

UNDER the title "A New Era in Interior Lighting," Mr. Charles L. Norton writes in the *Technology Quarterly* advocating the use of ribbed, corrugated and prismatic glass windows for diffusing light in the interior of rooms and offices. The only comment we can make is that as modern civilisation compels men to work in dingy offices and factories it has been necessary for modern civilisation to devise some means of lessening their dinginess, then "adaptation to environment" will come in and give us a civilised race which actually prefers this kind of illumination to that of the good old plate glass window. Of this tendency Mr. Norton himself affords an instance when he expresses the view that it is to be *regretted*, but it is certainly true, that strong objection is often made to the "shut in" feeling which some people experience in rooms glazed wholly with diffusing glass. He also considers it one of the uses of the diffusing window that it allows of the closer approach to one another of tall buildings, with a resulting economy of land—and, we should say, an aggravation of the unnatural conditions under which human life maintains its unlovely struggle for existence in densely populated centres.

IT is interesting to notice how the naval architect is becoming more and more dependent on a knowledge of applied mathematics and mathematical physics for the solution of the problems involved in perfecting the construction of steamships. It is only recently that the balancing of marine engines has received serious attention, and this problem has brought the principles of rigid dynamics as well as Fourier's series under the notice of the shipbuilder. But when the parts of an engine have been balanced on the hypothesis that they are perfectly rigid there still remain the effects of their elasticity to be taken into account. Mr. J. H. MacAlpine has recently communicated to the *Journal* of the American Society of Naval Engineers a monograph of 288 pages on "Inertia Stresses of Elastic Gears." The investigation seems to have been suggested, in the first instance, by the defective working of certain forms of valve gear. While Mr. MacAlpine hardly thinks that the elaborate processes of calculation which he gives can be frequently repeated in the ordinary course of designing, they might, at least partly, be resorted to with advantage and with but little labour, in cases where the effect of elasticity seems doubtful. Their application would have saved many expensive breakdowns in the past, and if applied to such cases as the *Newark*, where serious trouble has arisen, would gradually accumulate a store of valuable data which could not fail to be useful.

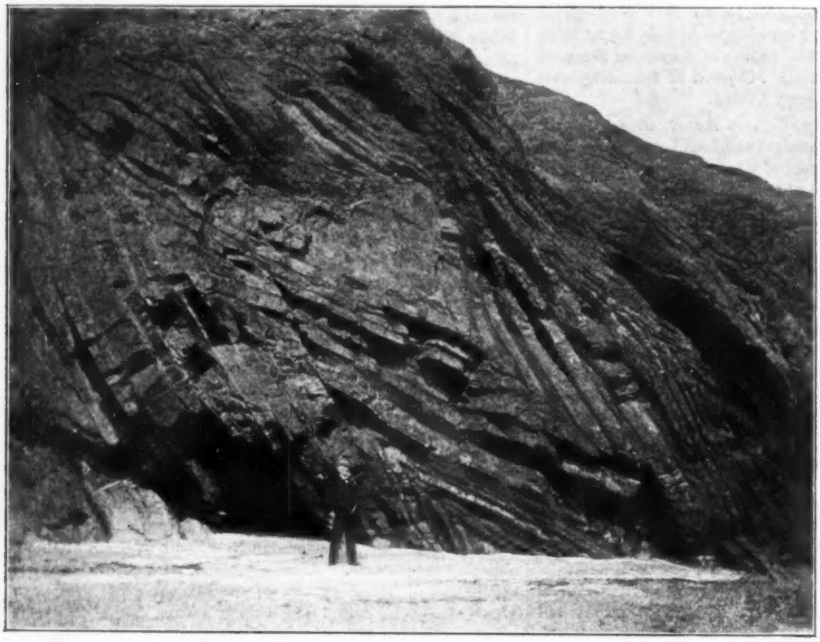
IF we may judge by the Report of the Marlborough College Natural History Society for 1900, the issue of the Victoria series of County Histories is having a good effect on institutions of this nature in calling attention to the incompleteness of their records of local faunas. In this particular instance, the local lists of the popular groups of Hymenoptera, Lepidoptera and Coleoptera were found to be well worked up, but those of other groups of insects had been much neglected. The editor also calls attention to the advisability of schoolboys confining their attention to a single section of zoology; otherwise, with the multitude of other studies and occupations, any real progress is impossible.

WE regret to learn, from a communication by Mr. A. J. North to the Records of the Australian Museum for 1901, that the destruction of native birds in New South Wales is attaining alarming proportions. After referring to a recent newspaper article containing an account of the slaughter of about 250 lyre-birds by one man during a single season, the author dwells on the injury done to bird-life in Australia by the growth of the great cities and their suburbs and the consequent clearance of timber and coppices. In Sydney the diminution in the number of indigenous birds owing to this cause is bad enough, but it is nothing to what has occurred in Melbourne, which is virtually denuded of trees for miles around. But this is by no means all, for the introduction of foreign mammals has played havoc with many kinds of native birds. Now that the rabbits have been eradicated in many districts the cats introduced to prey upon them have turned their attention to the birds; and the introduced foxes, in addition to robbing hen-yards, destroy hosts of indigenous birds. Neither can the sparrow and the starling be exonerated from blame in the matter. Mr. North urges the necessity of the duty of bird-protection being taught in the schools, as in the United States.

THE significance of spiral swimming—that is to say revolution on their own longer axis—by many of the lower organisms, such as the ciliate and flagellate infusorians and volvox, is discussed by Dr. H. S. Jennings in the May issue of the *American Naturalist*.

The function is considered to be of considerable importance. It has been found that the same side of the organism is always directed towards the outer side of the spiral. In the case of spherical organisms like volvox the spiral movement probably serves merely to correct any accidental deviations from a straight course; but without this device many creatures would be quite unable to steer straight, and many of them would merely describe circles without making any forward progress at all. "The simple device of revolving in the axis of progression is surprisingly effective, in that it compensates with absolute precision for any tendency, or combination of tendencies, to deviate from a straight course in any direction."

WE have received three specimen numbers of a popular Danish illustrated weekly magazine called *Frem* (Forwards), and devoted to ancient and modern history, archæology, literature and science. The outer portions of the paper are in quarto, but the inner portion, when cut up, consists of an octavo sheet, containing four pages each of various independent works. The parts before us, published in September and October, 1900, include parts of a novel; a translation of Shakespeare's "Henry IV."; a work on ancient history by Johan Ottosen, with illustrations of buildings, a cross, &c.; and a work by Levyson on the human body, with numerous text illustrations, and coloured diagrams of the organs of the upper part of the body and of the heart. The quarto contents of the parts are equally varied, and among them we notice articles and illustrations relating to the tortoises of the Orinoco, a Khirghis mother and children, edible and poisonous fungi, runes, old buildings, gout, the Moloch lizard, the Franco-German war, China, and the Transvaal, &c. It is one of the most miscellaneous publications which have come under our notice, in some respects resembling the old *Penny Magazine* of sixty years ago. *Apropos* of Shakespeare, we may mention that the plays are being translated into Finnish; and seventeen have already been published by the Finnish Literary Society at Helsingfors.



Contorted Beds at Jangye-Ryn, Gunwa

THE *Journal of Botany* for June gives the more interesting notes contained in the Botanical Exchange Club Report for 1899, which is now edited by the Rev. W. R. Linton.

MR. F. N. WILLIAMS has issued a specimen of a "Prodromus Floræ Britannicæ," in which an attempt is made to epitomise the distinguishing characters of all British species and subspecies of plants, the descriptions being given in Latin, "the nominative absolute style with separate sentences."

THE Royal Geological Society of Cornwall has lately issued its eighty-seventh Annual Report, together with the papers read during the session 1889-1890 (*Transactions*, vol. xii. part 6). Mr. J. B. Hill, who is engaged on the Geological Survey, has brought the experience which he gained in Argyllshire to bear on the slaty rocks of Cornwall. He finds the structures there to be identical with those of crystalline schists, but the mineralisation is wanting. In the Falmouth district the strata have been thrown into a series of isoclinal folds accompanied by small faults, and further minor structures have been set up until the mass has become full of minute folds and thrusts. These disturbances have in some cases caused, not only severing and brecciation of the bands, but also the rounding of fragments so as to produce "crush-conglomerates." The author remarks that had the rocks been subjected to these stresses at a greater depth and below the zone of fracture, where they would not have been so free to move, they would have been converted into true schists. Mr. Howard Fox gives a brief description of the remarkable contorted beds of Gunwalloe, in the Lizard district near Helston, together with an excellent photographic plate (which we reproduce by permission of the Society). The thick pale bands are grits, the thin dark bands are much squeezed shales, and there are numerous quartz and calcite veins. The beds appear to belong to the same group as the Ordovician cherts of Mullion Island. In an article on the sequence of the Lizard Rocks, Mr. Harford T. Lowe brings forward evidence to show that the granulitic series is later than, and instrusive in, the serpentine.

WE have received the first number of a new botanical journal, to be issued at irregular intervals, *Biltmore Botanical Studies*, embracing papers by the director and associates of the Biltmore Herbarium, North Carolina. The present number is occupied by five papers on descriptive phanerogamic botany.

IN a new edition of "Modern Cremation," which has just been published by Messrs. Smith, Elder and Co., Sir Henry

Thompson adds some important matter to the previous edition, and brings the history of the practice of modern cremation up to the present time. The case for cremation or some method of disposing of the dead other than burial is given much support by the evidence described in this book. The practical details given in an Appendix will be of service to people seeking information upon the subject.

MESSRS. DULAU AND CO. have sent us a copy of their various catalogues of zoological and palæontological books and pamphlets issued between 1896 and 1901. These, which are arranged in subjects, have been bound together into one volume, which will be found of considerable use to the working naturalist as a guide to much of the literature of any subject on which he may be engaged.

A NEW edition, revised and enlarged, of Prof. W. C. Unwin's "Elements of Machine Design" (Part 1) has just been published by Messrs. Longmans, Green and Co. The plan and general arrangement of the book remain the same as the original, published many years ago, but about a hundred pages have been added and numerous alterations have been made.

A NEW edition of "Telephone Lines and their Properties," by Prof. W. J. Hopkins, has been published by Messrs. Longmans, Green and Co. Among the additions are an account of the latest developments in the design of long lines, a chapter on "composite" working and wireless telephony, an abstract of Dr. Pupin's paper on telephony over cables and long-distance air lines, and a paper on inductive disturbances in telephone circuits.

RECENT numbers of American geographical journals contain much information about Alaska. In the May issue of the *National Geographic Magazine* Mr. Henry Gannett publishes an article on the general geography of Alaska. The second number of *Mazama* is devoted almost entirely to Alaska; it includes an account of the Harriman Alaska Expedition and a reproduction and explanation of an Indian map from the Chilkaht to the Yukon. *Mazama* also contains a paper on the flora of Mount Rainier, by Prof. C. V. Piper.

THE value of "The Statesman's Year Book" (Macmillan and Co., Ltd.) can only be rightly appreciated by those who keep the annual at hand for ready reference. The edition for 1901 has now appeared, and Dr. Scott Keltie and his colleague, Mr. Renwick, are again to be congratulated upon its publication. The work is an epitome of political geography, containing the essential particulars concerning the constitution, communications and commerce of every country in the world. The changes of the past year have necessitated the revision of several parts of the book. The Transvaal and the Orange Free State are now included in the section on the British Empire, and the Australian Commonwealth is described. The results of the censuses taken during last year and the early part of this are also given. There are five maps, the first giving a comparative view of geographical knowledge and political divisions in 1800 and 1900, and the second showing the political partition of Europe in the same years. The other maps represent railways, navigable waters and steamship routes in North America, South America and Australia. The volume now extends to 1320 pages, and ought not to be much further increased in size or it will lose its present handy character.

THE additions to the Zoological Society's Gardens during the past week include a Chacma Baboon (*Cynocephalus porcarius*, ♂) from South Africa, presented by Mr. Geo. Blay; a Rhesus Monkey (*Macacus rhesus*) from India, presented by the Hon. Mrs. Morrison; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Colonel B. McCalmont; a Pin-tailed Whydah Bird (*Vidua principalis*) from West Africa, presented by the Hon. Mrs. Parker; two Ocellated Sand Skinks (*Chalcides ocellatus*), South European, presented by Mr. W. H. St. Quintin; two Common Vipers (*Vipera berus*), British, presented respectively by Mr. Gerald Leighton and Mr. John Wright; a White-collared Mangabey (*Cercocebus collaris*), two Yellow Baboons (*Cynocephalus babouin*) from West Africa, a Yellowish Capuchin (*Cebus flavescens*), a Brazilian Tortoise (*Testudo tabulata*) from South America, a Silky Marmoset (*Midas chrysoleucos*) from Rio Madeira, Brazil; two Pinche Monkeys (*Midas oedipus*) from Colombia, a Three-banded Douroucouli (*Nyctipithecus trivigatus*) from Guiana, three Serrated Terrapins (*Chrysemys scripta*) from North America, two Black Tortoises (*Testudo nigra*) from the Galapagos, a Black Iguana (*Metopoceros cornutus*) from the West Indies, a Common Chameleon (*Chamaeleon vulgaris*), a Basilisk Chameleon (*Chamaeleon basiliscus*, from North Africa, a Blue-tongued Cyclodus (*Tiliqua scincoides*), thirteen Black and Yellow Cyclodus (*Tiliqua nigro-luteus*) from Australia, four Green Lizards (*Lacerta viridis*), three Dark Green Snakes (*Zamenis gemonensis*), three Tessellated Snakes (*Tropidonotus tessellatus*), two Æsculapian Snakes (*Coluber longissimus*), a Four-lined Snake (*Coluber quatuorlineatus*), European; a Chained Snake (*Coluber catenifer*) from California, deposited; a Red Deer (*Cervus elaphus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

TWO NEW VARIABLE STARS.—Prof. W. Ceraski announces in the *Astronomische Nachrichten* (Bd. 155, No. 3718) the discovery of two new variables at the Moscow Observatory. The measures were obtained from photographs.

72, 1901 (Lyræ).

R.A. h. m. s.		Decl. ° ′ ″		
19 7 37·01	...	+33 10 12·6	...	1855·0
19 9 17·62	...	+33 14 38·1	...	1900·0

The brightness varies from the 10th to 12th magnitude, in a period of from 0·27–0·81 of a year. At present it is about the 11th magnitude, and is increasing.

73, 1901 (Scuti).

R.A. h. m. s.		Decl. ° ′		
18 46 19·7	...	−12 46·9	...	1855·0

This variable is of the Algol type; normal magnitude about 9·0. Its period is about 22·9 hours, and its brightness varies from 9·1 to 9·6 in five hours. There appears to be evidence of two principal minima separated by a secondary one.

UNIFORM TRANSMISSION OF ASTRONOMICAL TELEGRAMS.—Prof. H. Kreutz, of the Central Astronomical Telegraph Bureau at Kiel, has issued a circular in several languages suggesting instructions for securing the adoption of a uniform system for the transmission of astronomical telegrams from the various observatories of Europe to the central bureau for subsequent general circulation.

The code suggested is very similar to that already in use for the telegrams which have been sent out from Kiel for several years past. A definite order is agreed on for the descriptive items of object, discoverer or observer, time, position, magnitude, motions and remarks, with a terminal number to control the accuracy of the numerical part of the message. In the circular issued examples of various possible forms of messages are given, both at length and in code, dealing with the discovery of comets or planets, new stars, orbits of comets, ephemerides, &c., perusal of which will easily make the scheme clear.

PHOTOGRAPHY OF CORONA.—In a reprint from a paper read before the Photographic Society of Philadelphia on March 13, 1900, Mr. H. W. Du Bois draws attention to the possibilities of the method, outlined by Prof. Nipher, of developing a positive from a plate which has received great over exposure, in connection with the problem of the daylight observation of the solar corona.

ELECTRO-MAGNETS.

IN this article it will be shown what a great advantage results from constructing electro-magnets on scientific principles, instead of making them according to everyday notions, and to give an idea which is the best form to adopt for producing very strong magnetic fields.

To understand the matter we must first consider the magnetic circuit, which is very analogous with the simple electric circuit. Just as when we have an electric current flowing in a copper rod, say, we know that the current is flowing round a complete circuit of which the rod forms only a part, so in the case of an iron rod magnetised by a current flowing round it, we consider that magnetism flows round a complete circuit of which the iron rod forms only a part. Fig. 1 is an ordinary bar electro-magnet; and in Fig. 2 B represents a cell and C A a copper rod the ends of which are joined by a great many thin wires of high resistance.

Now the flow of current in Fig. 2 is exactly analogous with the flow of magnetism in Fig. 1. The cell replaces the coil of

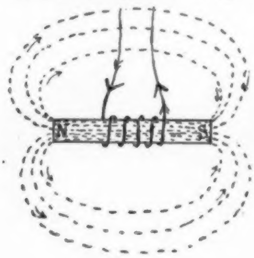


FIG. 1.

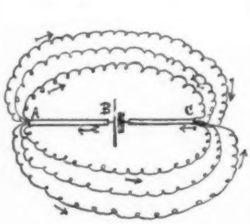


FIG. 2.

wire on the magnet, for whereas the cell sends the current in Fig. 2, the current flowing through the coil sends the magnetism in Fig. 1 through the iron rod, corresponding with the copper rod, and to complete the magnetic circuit the magnetism passes through the air in paths shown by the dotted lines, Fig. 1, back to the south pole of the magnet; so that the magnetic circuit in this case is formed partly of iron and partly of air.

The current flowing in the coils of wire on the magnet produces what is called a "magnetomotive force," which is proportional to the current and to the number of turns of wire; and a certain fraction of this quantity is used to send the magnetism through the iron rod and the remainder to send it through the air, or, in other words, every little piece of the magnetic circuit requires a certain magnetomotive force to drive the magnetism through it, and the sum of all these, taken all round the circuit, is the whole magnetomotive force due to the current in the coils; just as a certain part of the electromotive force of the cell is used to send the current through the copper rod and the remainder to send it through the thin wires forming the rest of the circuit. In fact, even the law governing the production of magnetism in a magnetic circuit is very similar to Ohm's law for the flow of current in an electric circuit, namely, that the amount of magnetism produced is equal to the magnetomotive force producing the magnetism, divided by the magnetic resistance, or "reluctance," as it is called, of the entire magnetic circuit. Hence, if the amount of magnetism is to be as large as possible it is just as important that the reluctance of the entire circuit should be small as it is that the current and number of turns of wire be large.

Now the reluctance of any little bit of a magnetic circuit, say from S to N, Fig. 1, for example, is proportional to the length of the piece, inversely proportional to its cross section, and also inversely proportional to the magnetic conductivity, called permeability, of the material. Therefore, to make the reluctance of our circuit small, we have to make: (1) its length small, (2) its cross section large, (3) and make it of a material whose permeability is as large as possible.

But the important thing is that the reluctance of the whole of the magnetic circuit must be small, not only of any particular part of it. For example, in Fig. 1, making the diameter of the iron bar large simply makes the reluctance of the circuit from S to N small, while the reluctance of the rest of the circuit, from N through the air to S is still very large, because the permeability of air is very small compared with that of iron. But if the bar is bent round into a ring, Fig. 3, then the reluctance of the whole circuit is reduced, and consequently a larger amount of magnetism will be produced in the bar for the same current flowing round it, and the density of the flow—that is, the strength of the magnetic field in the air space—will be very much increased.

There seems to be a popular idea that if a magnet is to produce as strong a field as possible it must be wound with an enormous number of turns of wire and a very large current sent round the coils, and that nothing else is of the least consequence. The following is a description of a large electro-magnet, made only about three years ago, which well illustrates this. It formed part of an electrical instrument intended to be used in connection with submarine telegraphy, the sole function of the magnet being to produce a very strong magnetic field. This result was certainly not obtained because it was not properly designed.

The magnet consists of two iron cores, 6 centimetres in diameter, each wound with about 1500 turns of wire, making the outside diameter more than 16 centimetres. To illustrate the

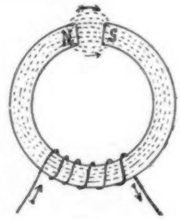


FIG. 3.

uselessness of this great amount of wire compared with the cross section of the iron, it has been found by experiment that if only one-third of the ordinary current is sent round the coils the strength of the magnetic field is thereby reduced only 15 per cent. If therefore the magnet had been wound with one-third the number of turns the cost of materials would have been about halved, and the power used to excite it only one-third, and even a less reduction than 15 per cent. in the strength of the field would have resulted.

The cross section of the piece of iron joining the two cores, *i.e.* "the yoke," is less than half that of the cores, and consequently the density of the flow of magnetism in the yoke is very large, and this means that the yoke will offer a great resistance to the magnetism for two reasons: (1) because the area is small, (2) Because the density being so large the permeability will be very small, for the magnetic conductivity gets rapidly less the greater the density; in fact, in this case the reluctance is so large that when the magnet is excited with its ordinary working current it produces a field of only 7900 C.G.S. units, and it can be calculated that the magnetomotive force used to send the magnetism through the yoke is then more than four times that required for the air gap, whereas in a properly designed magnet nearly all the magnetomotive force is used to send the magnetism through air gap and pole pieces. Doing what has been done here is exactly analogous with trying to send the strongest current that you can through an electrical apparatus by connecting to it the most powerful battery obtainable with two very long thin high-resistance wires. Analogy, therefore, shows us that the cross section of the yoke should have been made at least equal to that of the cores.

In order to see what sort of saving might have been effected, I have designed a magnet (Fig. 4) to produce the same effect as this one.

It consists of a cast steel ring, rectangular in section, the wire being wound on ten bobbins made of thin wrought iron, and not straight on the ring, for convenience in winding.

The design is made by starting with the assumption that a magnetic field exists of the strength desired in an air gap of the dimensions of the last magnet. Then the flow of magnetism at the section *a a*, Fig. 4, is calculated, ditto for section *b b*, where it is greater than at *a a*, by the amount which leaks out of the iron between these two sections. Similarly, the flow is obtained at all the sections, *c c*, *d d*, &c., round the circuit, the area of the iron at all these sections being made such that the density of flow has a value for which the magnetic conductivity

of the cast steel is high. When the density of the flow is found, having a previous knowledge of the magnetic quality of the steel we can get the permeability for each section. Then the product of current, into number of turns of wire, necessary to force the magetism through all the different sections into which the magnetic circuit has been divided can be found, and, adding all these together, we can obtain the current that must be sent round the magnet a given number of times to produce the desired effect. The following table gives the flow, density of flow, &c., at the different sections (Fig. 4), the "ampere turns," *i.e.* current, into number of turns, given in column 5, being for the particular section, together with the similar one, on the other side of the ring.

For Magnet, Fig. 4.

Section at.	Flow of mag. C.G.S. units.	Area of section, sq. cm.	Density of flow, C.G.S. units.	Current × No. of turns required for section. Ampere turns.
			For air gap	6360
aa	32,000	4·0	8,000	very small
bb	64,000	7·2	8,900	120
cc	126,000	7·2	17,500	70
dd	147,000	12·9	11,400	130
ee	200,000	12·9	15,500	320
ff	225,000	12·9	17,500	140
gg	249,000	18·0	13,800	160
hh	270,000	18·0	15,000	170
ii	270,000	18·0	15,000	
			TOTAL ...	7470

Column 2 shows what a large amount of magnetism leaks out from the one side of the ring and passes over to the other, not passing through the air gap at all. Column 3 shows how the

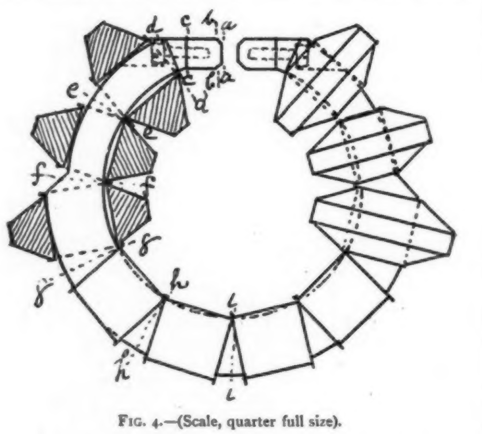


FIG. 4.—(Scale, quarter full size).

area of the iron has to be increased, owing to this leakage, for sections further away from the air gap. The current for this magnet is to be 5·4 amperes, and this flowing 1,500 times round should allow an ample margin to produce the field required.

It is interesting to compare the leading particulars of these two magnets.

Table comparing Two Magnets.

	Original magnet.	New design, Fig. 4.
Strength of field, C.G.S. units	7900	8000
Length of air gap, cm.	1	1
Area of air gap, sq. cm.	4	4
Exciting current, amperes	15	5·4
Turns of wire	3000	1500
Product of current, into number of turns, ampere turns	45,000	8100
Length of wire, yards	1140, No. 11	450, No 16.
Weight of wire, lbs.	139	17
Weight of iron in magnet, lbs.	41	15½
Total weight, iron and copper, lbs. ...	180	33
Cost of iron and copper— Iron @ 2·2*d.* per lb.; copper @ 1*s.* per lb.	£11 9 0	£2 11 0
Cost of working magnet continuously, per year, with current at 6*d.* per Board of Trade unit	£130 0 0	£25 0 0

Since the magnet was intended to work in connection with a submarine cable, it is quite safe to say it would require current throughout the year, and then, as the above table shows, there would be a saving of more than 100*l.* a year by using the magnet, Fig. 4, if the current could be got at 6*d.* per Board of Trade unit—it would, however, very likely cost much more under these conditions—to say nothing of the saving in first cost.

Another example of the importance of having sufficient iron in the magnetic circuit is afforded by the alterations that were made to an electro-magnet belonging to the Central Technical College. The cross section of its yoke was less than half that of the cores, due to the fact that the magnet was made before the theory of the magnetic circuit was understood. Recently a new yoke of proper cross section was made for the magnet, and it was found by experiment before and after the alteration that under precisely the same conditions the strength of the field had been just doubled simply by adding a few pounds of iron in the right place.

But at the same time it should be remembered that the magnetic properties of different specimens of iron vary enormously, and it is important that any iron which is to be used for a magnet should first be tested. Fig. 5 shows a very convenient apparatus for doing this. It consists of a massive iron frame into which you can slide a bar of the sample to be tested, the bar passing through a thin brass tube on which a known number of turns of wire are evenly wound. Also a few turns of fine wire are wound on the middle of the tube, shown at T, Fig. 5,

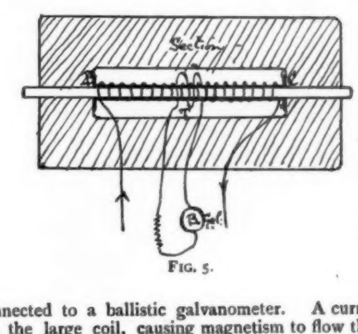
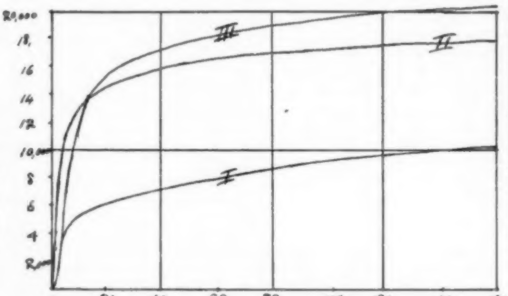


FIG. 5.

and connected to a ballistic galvanometer. A current is sent through the large coil, causing magnetism to flow through the bar, returning by the massive frame the cross section of which is so large that practically the whole of the magnetomotive force due to the current is used to send the magnetism through the bar; therefore, dividing the total ampere turns by the length of the bar we obtain the ampere turns necessary to send the magnetism through one centimetre of that specimen of iron. Several different strengths of current are sent through the magnetising coil, and in each case the flow of magnetism produced is measured by suddenly switching off the current, consequently

causing the magnetism passing through the secondary circuit connected to the ballistic galvanometer to rapidly die out, and in doing so a quantity of electricity, proportional to the amount of magnetism, is sent through the galvanometer, thus giving a measure of the amount of magnetism. Fig. 6 shows the difference between three specimens of iron—I. cast iron, II. wrought iron, III. best cast steel for magnets.

The curves show how rapidly the magnetic resistance of iron rises as the density of magnetisation is increased, and therefore the importance of not allowing the density to exceed about



$B = \frac{\text{Flow of magnetism}}{\text{Area of bar}} = \text{Density of flow.}$

$\frac{\text{Current into number of turns}}{\text{Length of bar, BC}} = $ Ampere turns per cm. length.

FIG. 6.

16,000 C.G.S. units. These curves are very useful in designing, because from them the ampere turns necessary to produce a certain density of magnetisation in a particular kind of iron can at once be found. It is most important to use the best steel, such as curve III. represents, for making magnets that are to produce very strong fields. With regard to what is meant by a very strong field, a field up to 20,000 C.G.S. units is moderately easily reached. But at about this limit saturation of the iron sets in and it becomes much more

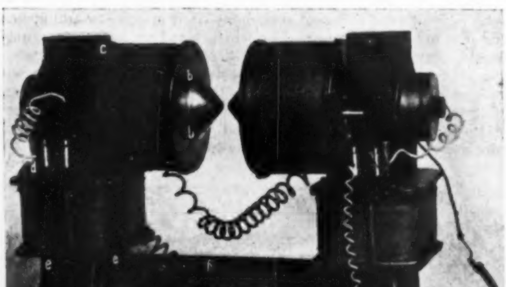


FIG. 7.

difficult to go higher. A field of 30,000 units is a very strong field; the cost of the magnet rises very rapidly if a stronger field than this is required. A field of 40,000 is about the strongest field obtainable. To go above this would require such a large additional expenditure in materials and power, compared with the small increase in the field, that it is not practical.

Fig. 7 shows a photograph of a good type of magnet to produce a very intense field. In this magnet the density of flow in the pole pieces is very great, so great, in fact, that the permeability of the pole pieces is not much greater than that of air; consequently a very large leakage of magnetism occurs, which makes the calculations very laborious. An idea of the amount of leakage that occurs is got from the fact that the flow of magnetism in the lower cylinder, Fig. 7, is fifty times greater than the flow across the air gap under working conditions, whereas in the case of the magnets of dynamo machines the flow in the yoke would not generally be more than 1·4 times the flow in the air gap, because they produce only relatively weak fields of about 6000 units. Owing to this great leakage the calculations cannot be made so accurately for this magnet, Fig. 7, as for dynamo magnets. In making the final calculations for this magnet it was necessary to divide the magnetic circuit in the pole pieces up into a great many sections, only two millimetres apart, finding the ampere turns necessary for each section, because the density changes so rapidly in the pole pieces, and also because the ampere turns required for the pole pieces are much greater than those required for the air gap, which cannot be helped, because the magnet was designed to produce a very intense field in a small air gap.

The following table gives the results of the final calculations:—

For Magnet, Fig. 7.

	Distance of section from pole tip in cm.	Section at.	Total flow of magnetism at section.	Area of section, sq. cm.	Density of flow of mag., C.G.S. units.	Product of current, into number of turns required for each section.
					For air gap 8900	
In pole piece	0	—	27,500	0·785	35,000	4,340
	0·2	—	53,000	1·54	34,400	3,500
	0·4	—	81,500	2·7	30,100	4,140
	0·8	—	139,500	5·7	24,500	763
	1·2	—	208,500	10·5	20,000	175
	2·2	—	375,500	27·3	13,800	24
	3·2	*bb*	535,000	52·75	10,150	660
		cc	925,000	52·75	17,500	183
		dd	1,054,000	80·0	13,100	540
		ee	1,206,000	80·0	15,100	894
		ff	1,374,000	85·0	16,100	
					TOTAL...	24,119

This magnet was originally intended to produce a field of 35,000 C.G.S. units, in an air-gap ⅛-inch long, for which the above calculations were made; but they show that the magnet will not be able to produce so strong a field as this, because it is wound so that it may be connected straight on to 200-volt mains, with all coils in series, or 100-volt mains with two sets of coils in parallel, and then there are 22,400 ampere turns available, whereas at least 24,120 are required.

Recently this magnet was made by the Electric Construction Company for the Solar Physics Laboratory, South Kensington, and the strength of the field produced under the conditions assumed in the above calculation was found to be 32,000 C.G.S. units, showing that the theoretical calculations agree fairly well with practice, considering the very high value of the field.

When the length of the air-gap was reduced to 2 millimetres, and the exciting current doubled, the maximum strength of field attained, for a short time, was nearly 38,000 C.G.S. units, which corresponds to a pull, between the pole pieces, of no less than 830 pounds per square inch !

T. L. JAMES.

SOME RECENT WORK ON DIFFUSION.[1]

THE subject of my lecture is one which, though essentially of a physical nature, had its origin in what may be regarded as a no man's land, a strip of neutral territory which can be claimed exclusively neither by the physicists nor the biologists.

An attempt to reconcile some apparently contradictory facts connected with the nutrition of plants has led, somewhat unexpectedly, to an extension of the laws of gaseous diffusion, so that we shall have to deal with one of those comparatively rare cases in which biology has been able to react to some extent on physics.

It has long been known that the primary source of the carbon of all plants is the carbonic acid existing in small quantities in ordinary atmospheric air, and that their green parts, more especially the leaves, are able to utilise the energy of sunlight in decomposing the carbonic acid and water and building up from their elements a whole series of substances, such as sugars and starch, which contribute directly to the nutrition of the plant.

The immediate seat of this synthetic and assimilatory process is found in the minute green chlorophyll granules which occur in great numbers within the cells of the leaf tissue, and one of the first problems to be dealt with in the study of the process is to show in what manner the highly dilute carbonic acid of the air can gain entry into the leaf with sufficient rapidity to supply these assimilating centres with material for the needs of the plant.

In a typical leaf, such as is represented in section in the diagram, both sides are covered with a cuticle and epidermis pierced at regular intervals on one or both sides with extremely minute openings, whose size is capable of being regulated according to the requirements of the plant. These are the *stomates* which open out into a relatively large cavity within the leaf, and this cavity in turn communicates with the numerous and roomy air-spaces between the cells containing the green chlorophyll granules.

One of the most important functions of the stomates is undoubtedly to regulate the transpiration of water from the plant, but the question of how far these minute openings play a part in the interchanges of gases between the interior of the leaf and the outer air has been a subject of very lively controversy.

It is now about thirty years since the eminent French chemist, Boussingault, came to the conclusion that the carbonic acid of the air gains access to the leaf, not through the *stomates*, but through the continuous substance of the cuticle and epidermis, by a process of *osmosis* similar to that by which carbonic acid had been shown by Graham to pass through a thin film of india-rubber.

So convincing did Boussingault's experiments and arguments appear to his contemporaries that this view became an article of faith for something like a quarter of a century, until, in fact, some five or six years ago, when Mr. F. Frost Blackman took up the subject and proceeded most inconsiderately to shatter all the most cherished statements of our text-books on this question.

I regret that time will not allow me to do more than state the general conclusions at which Mr. Blackman arrived and which may be briefly summarised as follows:

In the first place there is no appreciable passage of atmospheric carbonic acid through the surface of a leaf which is naturally devoid of stomates, such, for instance, as the upper surface of a normal leaf, which is quite imperforate; neither is any entry of carbonic acid possible when the stomates have been artificially blocked or made to close spontaneously.

In addition to this, if a leaf has stomates on both surfaces the relative in-take of carbonic acid by those surfaces bears a distinct relation to the distribution of the stomates.

We can, in fact, no longer doubt that when a leaf is respiring or assimilating mere osmosis of carbonic acid through the substance of the cuticle and epidermis plays little or no part in the gaseous exchanges, and that whatever the exact nature of the process may be it must be carried on exclusively by the minute openings of the stomates.

Since anything like a mass movement of the air through these openings is out of the question, we must look to the phenomena of *diffusion* for the true explanation, and especially to that form of it which was first described by Graham as *free diffusion*, that is to say the natural tendency possessed by gases or liquids to form a perfect mixture when they are in contact with each other and there is no partition of any kind between them.

[1] Discourse delivered at the Royal Institution, Friday, March 22, by Dr. Horace T. Brown, F.R.S.

This spontaneous mixing is quite independent of any currents or mass movements of any kind, and is brought about by the gradual interpenetration of the molecules of the one gas or liquid by the molecules of the other.

As an example of this kind of diffusion I have here a cylinder which a few weeks ago was partly filled with 5 per cent. gelatine solution. After the gelatine had set, the cylinder was filled up with a highly-coloured solution of a copper salt, which you now see has permeated the jelly to a certain depth. There has been no mixing of the solutions in the ordinary sense of the word, for the gelatine is virtually a solid. The effect has been produced by the molecules of the coloured copper salt, by reason of their rapid movement in all directions, gradually penetrating into the spaces between the molecules of the gelatine layer. Given a sufficient length of time and there would be an equal partition of the coloured substance between the two layers.

Diffusion takes place, as is well known, much more rapidly with gases than with liquids. Had our cylinder contained, for instance, carbonic acid in the lower half and air in the upper, a complete mixing would have taken place in a comparatively short time, even if all convection currents had been prevented.

The classical researches of Graham on the diffusion of gases through thin porous septa established the general law that the rate of diffusion of the different gases, under identical conditions, varies inversely as the square roots of their respective densities. Graham's results, however, only acquaint us with the *relative* velocities of diffusion, whereas for the particular problem which we have before us we must know the *absolute* velocities of diffusion under strictly defined conditions.

It is mainly to the Viennese school of physicists, and especially to Prof. Loschmidt, that we owe our present knowledge of the actual rate of penetration of one gas by another in free diffusion.

By observing the speed with which different pairs of gases spontaneously mix in a tube, Loschmidt was able to deduce certain *absolute values* expressing the velocity of their interpenetration.

Some of these results for different pairs of gases are given in the diagram, the last column representing the "constant of diffusivity" expressed in centimetre-gram-second units.

Let us consider the constant for carbonic acid and air, which at 0° C. is ·142. This means that when air and carbonic acid gas are freely diffusing into each other, an amount of either gas corresponding to ·142 cubic centimetre will pass in one second of time across an area of one square centimetre when the partial pressure of the gas varies by one atmosphere in one centimetre of length.

Now when we come to apply these absolute values of diffusivity to the passage of the extremely dilute CO_2 of the air into the leaf stomates (whose dimensions can of course be determined), we find that free diffusion through these openings is apparently able to account for only a portion of the gas which we know must enter the leaf, unless we make some extremely improbable assumptions as to the very low point at which the partial pressure of the carbonic acid is maintained immediately under the apertures.

I shall not, however, trouble you with the calculations on which this statement is based, since I prefer to put the matter in a more concrete form, which has also the advantage of emphasising the extraordinary power which an assimilating leaf possesses of extracting carbonic acid from its surrounding air.

There are two methods by which we can determine the actual amount of atmospheric carbonic acid used up by an assimilating leaf, one a direct the other an indirect method.

Part of the apparatus used in the direct method is shown on the table.

The leaf, which may be still attached to the plant, is enclosed in a glazed case, through which a measured current of air is drawn of which the carbonic acid content is accurately known. When the air emerges from the case it passes through an absorption apparatus, which retains the whole of the CO_2 left in the air after passing over the leaf. This absorbed carbonic acid is determined at the close of the experiment, and we then have all the data for estimating the carbonic acid abstracted from the air by the leaf. The area of the leaf being known, the CO_2 absorbed can be referred to a unit area of leaf and a unit time.

By the indirect method, which is due to Sachs, the actual increase in dry weight of a given area of an assimilating leaf is determined, and since this increase in weight is due to

substances having a definite percentage of carbon a simple calculation enables us to determine the equivalent amount of carbonic acid abstracted from the air.

By such methods as this it can be shown that an actively assimilating leaf, such as that of the Catalpa tree, in full daylight, and under favourable conditions of temperature, can take in carbonic acid from the air at the rate of about 1/10th cubic centimetre per hour for each square centimetre of leaf.

Since there are only about three volumes of carbonic acid in 10,000 volumes of ordinary air, this must mean that in a single hour the under surface of the leaf will take in as much carbonic acid as is contained in a column of air about eight feet long, and having the same area of cross-section as the leaf.

But this remarkable power of an assimilating leaf will be better appreciated if we compare it with a liquid surface of a strong solution of caustic alkali, which is known to have such a great avidity for carbonic acid.

We can investigate the absorptive power of such solutions for the carbonic acid of the air under fixed and controllable conditions by using a form of apparatus which I have on the table, and which can be examined at the close of the lecture. It is so arranged that an air current of known velocity can be drawn over the surface of the absorbing solution which has a known area.

When a very low velocity of the air current has been reached the amount of absorption becomes constant at ordinary temperatures at about ·17 c.c. of carbonic acid per square c.m. of surface per hour.

So we see that a leaf, assimilating under natural conditions, is taking in carbonic acid from the air more than half as fast as a surface of the same area would do if it were wetted with a constantly renewed film of a strong solution of caustic alkali submitted to a strong current of air.

This is in itself a somewhat remarkable conclusion, but what are we to say to a proposition which would limit the absorptive power of the leaf surface to the extremely small apertures of the stomates?

In a leaf such as we have been considering, the aggregate area of the openings of the stomates, when expanded to their widest, amounts to less than *one per cent.* of the total leaf surface, so that if the entry of the CO_2 takes place exclusively by these openings we must conclude that it goes in more than fifty times faster than it would do if the mouth of each one of these minute openings were filled with a constantly renewed solution of strong caustic alkali.

Such facts make it difficult unreservedly to accept the view that the gaseous exchanges in leaves are really carried on exclusively by the stomates, which occupy such a small fraction of the leaf surface. On the other hand, the direct experimental evidence in favour of this view is overwhelming, so that we apparently find ourselves on the horns of a dilemma.

There appeared to be only one way out of the difficulty, that was to assume that the leaf knows more about the laws of free diffusion than we do, and has adapted itself to some physical principles which have hitherto escaped notice. This was found to be the case when the structure of the leaf was regarded as a piece of physical apparatus for promoting rapid diffusion.

I do not propose to take you through the various and tedious stages by which the true explanation was reached, but will attempt, as far as possible, to short-circuit the current of the argument.

In the first place I wish to call your attention to a particular mode of free diffusion which, in gases, has been but little studied, but which has a very direct bearing on diffusion in the living leaf, where one of the constituents of the diffusing gases, the carbonic acid, is very small in amount compared with the others.

Let us for a moment concentrate our attention on the air which is contained in this open glass cylinder, and endeavour to picture to our minds the jostling crowds of the perfectly elastic molecules of the various gases, flying hither and thither in all imaginable directions and coming into frequent collision with each other and the sides of the containing vessel.

Now in this jostling throng there is a certain proportion of molecules of *carbonic acid*, which we will imagine for the moment are distinguished from the molecules of the other gases by some difference in colour—let us suppose them to be *green*.

Now further consider a plane surface in the contained air of the cylinder; from the dynamical theory of gases it follows that in any given interval of time, temperature and pressure remaining constant, the same average number of the "green" molecules will cross this imaginary plane in opposite directions, and since this will be true for any plane surface, no matter where we take it within the cylinder, there can be no change in the average distribution of the "green" molecules throughout the cylinder—in other words, no change in any part of the cylinder in the composition of the air as regards its carbonic acid content.

But now let us imagine that the bottom of the cylinder is suddenly made capable of absorbing carbonic acid, say by the introduction, without any disturbance of the air, of a little solution of caustic soda or caustic potash. The "green" molecules which now strike the bottom of the cylinder at all imaginable angles of incidence will not all rebound as they originally did, but will be to a large extent trapped in their to and fro excursions, so that in the very first brief interval of time a very thin stratum of air, parallel to and immediately above the absorbing surface, will be partially freed from its "green" molecules.

Now consider the kind of exchange of "green" molecules which occurs in the next very brief interval of time between this partially depleted layer at the bottom and the one immediately above it. The rate of exchange across the imaginary plane dividing these two contiguous layers can no longer be equal and opposite since the number of "green" molecules in the upper stratum is greater than that in the lower. A larger number of the "green" molecules must consequently pass in a given brief interval of time from the higher to the lower stratum than from the lower to the higher; in other words, the *balance of exchange* is in favour of the lower layer. This state of affairs will rapidly propagate itself upwards until the mouth of the cylinder is reached, and, provided the air outside the cylinder is kept of the same composition and the absorptive power of the bottom of the cylinder is also kept constant, these *uncompensated balances of exchange* between the imaginary layers may be regarded as constituting a steady *flow* or *drift* of the "green" molecules down the tube towards the absorbent surface.

Although within the column there is this constant flow of carbonic acid molecules in the general direction of the axis of the tube, the system as a whole may now be regarded as static as long as all the conditions remain unchanged. The flow is, then, strictly analogous to the "flow" of heat in a bar of metal which is kept with its two ends at a uniform difference of temperature, or to the flow of electricity in a conductor between two regions maintained at a constant difference of potential; and static diffusion admits of precisely the same simple mathematical treatment as these phenomena of conduction of heat or electricity when we come to its quantitative study.

In such an imaginary experiment as we have been considering it is clear that the amount of carbonic acid in the air of the cylinder must vary uniformly from a maximum at the top of the cylinder to a vanishing point at the bottom, so that if the CO_2 really had the green colour which, for purposes of argument, we have attributed to it, the depth of colour of the air column would uniformly diminish from top to bottom.

This can be illustrated by the diffusion of a coloured copper salt down a gelatine column. If this column were cut off just where the colour ceases to be perceptible, and the cut end were immersed in water to carry off the diffusing salt as fast as it came through the column, then if the upper end of the column remained in contact with the coloured copper solution we should ultimately get a constant steady flow of the salt down the column.

Under these conditions it can be readily shown, both experimentally and theoretically, that the actual amount of substance diffusing down the column in a given time will, in the first place, be directly proportional to the difference in the concentration of the diffusing substance at the two ends of the column; it will also be directly proportional to the *area* of cross-section of the column, but inversely proportional to its length.

The fact which for the moment I wish you to bear in mind is that, all other things being the same, the amount of diffusion down a column of this kind *varies directly as the area of the cross-section of the column*.

This is roughly illustrated by these two cylindrical columns of gelatine of different diameters, down which a coloured solution has been diffusing for equal times.

The salt has penetrated both columns to the same depth, and the gradation of colour is also the same, a proof that the rates of diffusion down the columns must be proportional to their areas of cross-section.

But now let us consider what will happen if instead of varying the width of the column throughout its entire length we only partially obstruct the cylinder somewhere in the line of flow, say by means of a thin diaphragm pierced with a single circular hole of less diameter than the bore of the tube.

We must resort to experiment to answer this question.

Suppose we take a series of exactly similar flasks, such as I have here, and produce a steady flow of atmospheric carbonic acid down their necks by partially filling each flask with a solution of caustic soda, the amount of carbonic acid entering the flasks being determined by subsequent titration of the soda solution. We can then study the effect produced by partially obstructing the mouths of the flasks with thin discs of metal or celluloid pierced with a single hole of definite size.

The results of a series of experiments of this kind are given in Table I., and you will see that under these conditions the amounts of carbonic acid diffusing down the cylindrical necks in a given time are not proportional to the areas of the apertures, as might reasonably have been expected, but are directly proportional to their *diameters*.

TABLE I.

Diffusion of Atmospheric CO_2 through single apertures of varying size.

Diameter of Aperture	CO_2 diffused per hour	CO_2 diffused per sq. cm. per hour	Ratio of Areas	Ratio of Diameters	R^a to of CO_2 diffused
mm.	c.c.	c.c.			
22·7	·2380	·0588	1·00	1·00	1·00
6·03	·0625	·2186	·07	·26	·26
3·23	·0398	·4855	·023	·14	·16
2·11	·0260	·8253	·008	·093	·10

This, of course, implies that as we make the aperture smaller the flow through a given unit of its area is proportionately increased; in other words, the acceleration of flow is *inversely proportional to the diameters of the apertures*.

This unexpected fact, which lies at the root of the whole question we are considering to-night, may be experimentally illustrated in a variety of ways.

We may, for instance, cause the aqueous vapour of the air to diffuse into a similar series of flasks, using in this case strong sulphuric acid as the absorbent, and determining the amount of diffusion of the water vapour by weighing the flasks from time to time. You will see from the results of such an experiment that the diffusion rates again follow pretty closely the ratios of the diameters of the apertures, and are widely divergent from the ratios of areas. (See Table II.)

TABLE II.

Diffusion of Aqueous Vapour through apertures of varying size.

Diameter of Apertures	Ratio of Areas	Ratio of Diameters	Ratio of Diffusion for equal times
mm.			
2·117	1·0	1·0	1·0
3·233	2·3	1·52	1·55
5·840	7·6	2·75	2·54

This "diameter law" is also applicable to circular liquid surfaces, the amount of absorption or evaporation from such surfaces varying, under certain conditions, not in accordance with the area of the surfaces, as might have been expected, but with their diameters.

I have here a short burette-like tube with a wide rim of metal round the top. When this tube is completely filled by letting in a solution of caustic soda we have a circular surface of the solution lying in the same plane as the rim. When this has been exposed to the air for a given time the carbonic acid absorbed by the disc of liquid can be determined by drawing off and titrating.

If such absorptive discs of different dimensions are exposed to air which is in *slight movement*, we shall find that the carbonic acid absorbed is proportional to the *area* of the surface. The smaller, however, we make the discs, and the greater precautions we take to keep the air over them perfectly still, the nearer do the absorptions become proportional to the diameters. (See Table III.)

There is always, however, more difficulty in obtaining these results with plane absorbing surfaces than by diffusion through a perforated diaphragm. The reason for this will be apparent later.

TABLE III.

Absorption of Atmospheric CO_2 by Circular Surfaces of Solutions of Caustic Alkali.

Diameter of Surface mm.	Ratio of Areas.	Ratio of Diameters.	Mean Ratio of Areas and Diameters.	Ratio of CO_2 absorbed.
10·25	1·0	1·0	1·0	1·0
20·25	3·9	1·9	2·9	3·0
29·25	8·1	2·8	5·4	5·3
40·00	15·2	3·9	9·5	9·2
5·0	1·0	1·0	—	1·0
10·25	4·2	2·05	—	2·47

Before entering on an explanation of these facts I wish you to note a very important conclusion to be drawn from them, and one which readily admits of experimental verification.

We have seen that when we partially obstruct the diffusive flow of a gas or liquid by a thin septum with a single circular perforation, the velocity of the flow through each unit area of aperture increases as the diameter of the aperture decreases.

One might, therefore, expect that if a number of fine holes were suitably arranged in such a septum, the acceleration of flow through the individual holes might assume such proportions that a perforated septum of this kind would exercise little or no obstruction on the diffusive flow, although in such a case the aggregate area of the holes might only represent a small fraction of the total area of the obstructing septum.

Strange and paradoxical as such a conclusion may at first sight appear, it will bear the test of experiment.

I have here a thin film of celluloid; in fact, a piece of the ordinary Kodak roller-film. This has been perforated with holes about ·4 millimetre in diameter, arranged at a little more than 2·5 diameters apart, so that there are just one hundred of such perforations on a square centimetre of area. The clear holes represent about 1/10th of the area of the film, 9/10th of the sieve being blocked up with impervious celluloid.

Here are two columns of gelatine, down which a blue solution of copper-ammonium sulphate has been diffusing for equal times. One of these columns is unobstructed in any way, being in direct contact with the coloured liquid. In the other case a finely perforated celluloid film has been interposed, which has the effect of blocking out 9/10ths of the cross-section of the column. You see that, notwithstanding this, there is no appreciable difference in the amounts of coloured salt which have diffused in the two cases; the salt has, in fact, gone through the finely-pierced septum as readily as if no obstruction were present.[1]

We find that exactly the same holds good with gaseous diffusion.

If finely perforated septa of this kind are luted on to short tubes containing caustic soda and are exposed to still air, the amount of carbonic acid diffusing through the holes in the diaphragm can be compared with the amount which we know would diffuse down the open tube under like conditions.

Some results of this kind are given in Table IV.

[1] The celluloid film is itself not permeable.

TABLE IV.

Diffusion of Atmospheric CO_2 through Multiperforate Septa into Tube 4 c.m. long. Diameter of Holes ·380 m.m.

No. of Holes per sq. cm.	Diameters Apart.	CO_2 Diffusing through Septum per hour c.c.	Open Tube Diffusion per hour c.c.	Percentage of Septum Diffusion on Open Tube Diffusion.	Percentage area of Cross-section occupied by Holes.
100	2·63	·361	·346	104·3	11·34
25	5·26	·148	·342	43·2	2·82
11·11	7·8	·131	·352	37·2	1·25
6·25	10·52	·110	·353	31·1	·70
15·7	15·7	·068	·334	20·4	·31

I must now ask you to follow me in a somewhat theoretical excursion in quest of an explanation of these curious facts.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The following have been elected public examiners:—Mr. R. T. Glazebrook, in physics; Mr. P. Elford, in chemistry; Prof. F. Gotch, in physiology.

The curators of the University Chest have been authorised to spend a sum not exceeding 1400*l.* in certain extensions of the Chemical Department which are necessitated by the loss of the laboratory known as the "Glastonbury Kitchen." The latter is now required as access to the new Radcliffe Library.

A proposal to permit candidates for the degree of Bachelor of Letters or Science to keep more than one term of University residence in the year by residence during the vacation has been rejected.

A proposal to provide access for wheeled traffic to the Departments of Physiology, Human Anatomy and Pathology at the back of the University Museum has also been rejected owing to the opposition of those who regard this as an encroachment upon the University Park.

The Junior Scientific Club held their 226th meeting on May 31. A paper was read by E. Walls, entitled "The Quest of the Philosopher's Stone." Prof. Silvanus Thompson delivered the Boyle Lecture on June 6, on "Magnetism in Growth."

CAMBRIDGE.—In the mathematical tripos, part i., the senior wrangler is Mr. A. Brown, of Caius College, a Ferguson student from Edinburgh. Miss Reynolds, of Newnham, is bracketed 11th wrangler. Three names appear in the first class of part ii.: Mr. J. E. Wright, Trinity (senior wrangler 1890); Mr. T. H. Havelock, St. John's (15th wrangler); and Mr. J. Chadwick, Pembroke (5th wrangler). Miss W. M. Hudson, Newnham, is in the first division of the first class (bracketed 8th wrangler 1890).

The professor of pathology announces ten separate courses of lectures and practical work to be given in the long vacation, beginning July 8.

PROF. R. W. WOOD, of the University of Wisconsin, has been appointed professor of physics in the Johns Hopkins University, in succession to the late Prof. H. A. Rowland.

WE learn from *Science* that the Wisconsin Legislature has granted 210,000 dollars to the University of Wisconsin, at Madison, in addition to the regular income previously derived from the State. Of this sum 150,000 dollars is for a new building for the College of Agriculture, which is to house the administration offices of this department and the experiment station as well as the departments of bacteriology and chemistry. This College also receives 10,000 dollars annual increase to its present income. The College of Engineering receives 30,000 dollars for equipment of its new building, which was provided by the last Legislature; also 7500 dollars annual increase in income. The newly organised School of Commerce secures 3500 dollars annual increase.

DR. H. M. KYLE has been appointed naturalist to the Marine Biological Association and fisheries' instructor for the county of Devon. Dr. Kyle is a distinguished graduate of the University of St. Andrews, having gained the rector's prize for an essay on evolution and having held successively the Fisheries' prize, the Berry scholarship (100*l.*) and, for three years, the Exhibition of 1851 scholarship (150*l.*) for original researches in connection with the fisheries. His studies for seven years have been devoted to marine zoology and the scientific treatment of the problems of the fisheries at the chief marine laboratories of Europe, including Naples, Plymouth, &c., and both the old laboratory and the new (Gatty) laboratory at St. Andrews, where he was trained.

WE learn from the *Berliner Klinische Wochenschrift* that the second annual congress of the German Association for School Hygiene, which was founded about two years ago for the purpose of studying and promoting all matters relating to health and hygiene in connection with schools, was held at Wiesbaden on May 31. The municipal authorities of that city placed the "Curhaus" at the disposal of the council of that association, where all the official meetings were held during the congress. The attendance was a large and a representative one, and the programme contained many important and highly instructive subjects, of which the following may particularly be mentioned: (1) the new Prussian school reform in relation to school-hygiene; (2) the hygienic condition of German schools in general, with special reference to that of Wiesbaden; (3) the prevention of infectious diseases regarded from a general point of view, with special reference to the spread of tuberculous affections amongst school children.

THE *Educational News* of Scotland states that the following is the list of candidates for the chair of natural philosophy in Edinburgh University, vacant through the resignation of Prof. Tait:—Prof. J. C. Beattie, South African College, Cape Town; Prof. G. H. Bryan, F.R.S., University College, North Wales; Dr. Charles Chree, F.R.S., National Physical Laboratory, Richmond; Dr. Cargill G. Knott, University of Edinburgh; Prof. J. P. Kuenen, University College, Dundee; Dr. Charles H. Lees, Owens College, Manchester; Mr. David B. Mair, Civil Service Commission, London; Prof. J. A. M'Clelland, University College, Dublin; Prof. J. G. MacGregor, F.R.S., Dalhousie University, Halifax, U.S.A.; Prof. Karl Pearson, F.R.S., University College, London; Mr. G. F. C. Searle, Cambridge; Mr. George W. Walker, Cambridge; Mr Gilbert T. Walker, Cambridge; Mr. C. T. R. Wilson, F.R.S., Cambridge.

PROF. RAMSAY expressed the views of a number of teachers and investigators in the annual oration delivered by him at University College, London, last week, on "The Functions of a University." The essential principle of University work should be research. This, said Prof. Ramsay, should be the goal to be clearly kept in view in the philosophical faculties of Universities. He was not one of those who would urge that a young man should not learn a great deal of what had been already discovered before he attempted to soar on his own wings. But there was all the difference in the world between the point of view of the student who read in order to qualify for an examination, or to gain a prize or scholarship, and the student who read because he knew that thus he would acquire knowledge which might be used as a basis of new knowledge. It was that spirit in which our Universities were so lamentably deficient; it was that spirit which had contributed to the success of the Teutonic nations, and which was beginning to influence the United States. A University which did not increase knowledge might be a technical school or a coaching establishment, but it had no claim to the name University. The best way of fitting young men for the manifold requirements of the Empire was to give them the power of advancing knowledge.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, May 16.—Prof. Emerson Reynolds, president, in the chair.—The following papers were read:—Derivatives of methylfurfural, by H. J. H. Fenton and Miss M. Gostling. A simple method of obtaining pure methylfurfural is described.—Optically active nitrogen compounds and their bearing on the valency of nitrogen; dextro- and lævo-α-benzyl-

phenylallylmethylammonium salts, by W. J. Pope and A. W. Harvey. The authors have prepared in a state of purity a number of substances owing their optical activity to the presence of an asymmetric nitrogen atom; it is shown that *d*- and *l*-benzylphenylallylmethylammonium iodides and bromides slowly become optically inactive when preserved in chloroform solution.—Reactions of hydroxyoxamides, by R. H. Pickard and W. Carter. Hydroxyoxamide and its phenyl-, tolyl- and naphthyl-derivatives give the general reactions of hydroxamic acids and are thus quantitatively convertible into substituted biurets and allophanates.—The *sym*-trichlorobromoanilines, and chloro- and bromo-amino-derivatives of chlorobromoacetanilides, by F. D. Chattaway and K. J. P. Orton. The authors call attention to the great resemblance existing between the two similarly constituted *sym*-chlorodibromoanilines, their acetylderivatives and the two *sym*-dichlorobromoanilines respectively.—Replacement of bromine by chlorine in anilines, by F. D. Chattaway and K. J. P. Orton.—The absorption spectra of cyanogen compounds, by W. N. Hartley, J. J. Dobbie and A. Lauder. An examination of the absorption spectra confirms the view that cyanuric acid and methyl cyanurate are similarly constituted, and indicates that the relations between melamine and triethylmelamine are correctly represented by the commonly accepted formulæ.—The nutrition of yeast. Part iii. By A. L. Stern. The author concludes that any increase of nitrogenous or inorganic nutriment beyond a definite limit will not increase either the amount of nitrogen assimilated by yeast or the weight of the yeast; any increase of the added sugar, however, is accompanied by an increase both in the amount of nitrogen assimilated and in the weight of the yeast. The growth of yeast proceeds as long as any sugar remains unfermented and is, during part of the fermentation, proportional to the amount of sugar fermented.—On the colloid form of piperine, with especial reference to its optical refraction and dispersion, by H. G. Madan. On cooling piperine, after heating to 180° for an hour, it remains in the colloidal state for an indefinite time; the colloid has a high refractive index ($\mu_D = 1{\cdot}684$) and exhibits an extraordinarily high dispersive power.—Note on pyromucylhydroxamic acid, by R. H. Pickard and A. Neville.—The condensation of ethylphenylketone with benzaldehyde, by R. D. Abell. Ethylphenylketone and benzaldehyde condense in presence of sodium ethoxide with formation of 1:3-diphenyl-2-methyltrimethylene glycol, benzalpropiophenone and 1:3-dimethyl-1:3-dibenzoyl-2-phenylpropane.—A new method for the determination of hydrolytic dissociation, by R. C. Farmer. The author's method of ascertaining the extent of hydrolytic dissociation depends upon determinations of the free acid or base by distribution between two solvents, one of which dissolves only one of the dissociation products.—The production of some new metallic borides, by S. A. Tucker and H. R. Moody. Crystalline borides having the compositions Zr_3B_4, CrB, WB_2 and Mo_3B_4 are prepared by heating the corresponding metal with boron in the electric furnace.—The action of lead thiocyanate on the chlorocarbonates. Part ii. Carboxymethyl- and carboxyamylthiocarbimides and their derivatives, by R. E. Doran.—The chlorine derivatives of pyridine. Part vii. Some condensation products, by W. J. Sell and F. W. Dootson.—The diazotisation of dinitroanisidine and the constitution of the resulting product, by R. Meldola and J. V. Eyre.

MANCHESTER.

Literary and Philosophical Society, May 28.—Prof. Horace Lamb, F.R.S., vice-president, in the chair.—The influence of grinding upon the solubility of the lead in lead fritts, by Dr. T. E. Thorpe, C.B., F.R.S., and Mr. Charles Simmonds. The paper was a criticism of the methods and conclusions contained in a paper by Messrs. Jackson and Rich, read before the Society in October last. The argument of that paper was stated to rest on the assumption that a fritt behaves as a single chemical compound—an untenable assumption. The theory that as a fritt is dissolved by acid a layer of silica is formed on the outside of the particles, protecting them from further action, was opposed as not being in accordance with facts which are easily demonstrated. The particular fritts used by Messrs. Jackson and Rich in their experiments were of somewhat high solubility, and the conclusions arrived at did not hold for those of lower solubility. A fine powder was, indeed, somewhat more soluble than a coarse one, but the variations of solubility of slightly soluble glazes between the limits of fineness occurring in actual practice were of inconsiderable magnitude. Further, whether or not the solubility varied to some extent with the fineness, the matter was of no practical consequence, since glazes could be obtained, and were in use, which were of the fineness used in working and conformed to the limit of solubility suggested by the Home Office. In the discussion which followed Mr. Burton pointed out that even if grinding only produced—as in experiments actually made with fritts of solubility below the Home Office standard—variations of solubility of some 50 per cent., a fritt not far within the limit would be dangerous in use or not according to the fineness of grinding. He also denied that the more soluble fritts are the softer, as alleged by Dr. Thorpe, but stated that the opposite was the fact. He referred to the danger of lead poisoning from inhaled lead dust, a matter in which solubility in dilute acid did not come into account. Mr. Jackson stated that the finer portions of the fritts dealt with by himself and Mr. Rich contained not more but less lead oxide than the coarser portions, contrary to the suggestions of Dr. Thorpe. He mentioned that he had himself found solubilities of from below 2 per cent. to about 5 per cent. from the same fritt at different grindings, the fritt being one which had been passed by Dr. Thorpe as within the Home Office limit. He showed some photographs of glasses acted on by hydrofluoric acid, showing crystalline forms suggestive of distinct heterogeneity even in the clearest glass, and stated that he had certainly not treated the fritts as single chemical substances.

PARIS.

Academy of Sciences, June 5.—M. Fouqué in the chair.—New researches on the neutralisation of phosphoric acid, by M. Berthelot. When an excess of a solution of lime is added to phosphoric acid, the calcium phosphate precipitated has at first the composition $Ca_3(PO_4)_2$, but in presence of an excess of lime a more basic salt is gradually formed, which finally approximates to the composition $H_3PO_4.2CaO$. An analogous compound has been observed in nature, the oxychloride $CaCl_2$, 3CaO. Similar compounds appear to be formed with baryta.—New researches on the alloys of gold and silver and of other materials arising from Egyptian tombs, by M. Berthelot. Analyses of fragments of gold of the eleventh, twelfth and thirteenth dynasties, of a supposed perfume, and of a copper alloy.—On the magnetic analysis of the radium rays and of the secondary radiation provoked by these rays, by M. Henri Becquerel. A development of the method described in a previous paper.—The physiological action of the radium rays, by MM. Henri Becquerel and P. Curie. Radiferous barium chloride carried on the arm in a thin guttapercha envelope caused at first a reddening of the skin resembling a burn, but without pain. After some days the area of this increased and the skin was broken, and fifty-two days after the action of the rays there still remained a sore. In another experiment with a more active material, the effect of the rays was felt through a glass tube containing the material, a box and the clothes. Inflammation with suppuration was produced in this case after only six hours' exposure to the rays, the wound produced not being entirely healed until forty-nine days after the exposure.—The changes in direction and velocity of an air current which has encountered bodies of divers forms, by M. Marey.—On regressive erosion in the chain of the Andes, by M. de Lapparent. Owing to the possibility of rapid variation of the watershed in this region, the line marking the watershed between the Pacific and Atlantic Oceans, as it exists to-day, does not constitute a true geographical boundary.—On the tellurides of gold and silver in the region of Kalgoorlie in Western Australia, by M. Ad. Carnot. Some analyses of the West Australian minerals sent to the Paris Exhibition. With the exception of some traces of mercury and copper these are practically double tellurides of gold and silver of the type $(Au, Ag_2)Te_2$.—On the longitudinal and transversal waves in perfect fluids, by M. P. Duhem.—Contribution to the theoretical and experimental study of liquid veins deformed by obstacles, and on the determination of the lines of induction in a magnetic field, by Prof. H. S. Hele-Shaw. A description of the author's method of photographing stream lines, with three examples. The method not only allows of the verification experimentally of many of the results deduced theoretically in hydrodynamics, but also furnishes a complete solution of many problems of practical importance which it is impossible to attack by mathematical analysis.—Determination of the surfaces which are at the same time surfaces of Joachimsthal and surfaces of Weingarten, by M. L. Raffy.—Observations on electric resonance in rarefied air, by M. Albert Turpain.—The influence of temperature on the electromotive force of magnetisation, by M. Rene Paillot. Using the method described in a previous paper, it was

found that the electromotive force of magnetisation of soft iron increases with the temperature, this variation with the temperature being greater as the field is more intense. With bismuth the opposite effect is observed, the electromotive force of magnetisation falling off as the temperature is raised.—The action of the X-rays on conductors and on insulators, by M. J. Semenow.—On the alloys of aluminium. Compounds of aluminium with molybdenum, by M. Leon Guillet. By reducing molybdic acid with a large excess of aluminium three definite compounds were obtained corresponding to the formulæ Al_4Mo, $AlMo$, Al_7Mo, analyses of which are given.—On the alloys of aluminium and magnesium, by M. Boudouard. A set of determinations of the melting points of thirteen aluminium-magnesium alloys ranging from pure aluminium to pure magnesium. The curve of results presents three minima and two maxima, pointing to the existence of two definite compounds, $AlMg_2$ and $AlMg$.—On the cellular structure of some metals, by M. G. Cartaud.—Acidimetry of phosphoric acid by baryta, strontia and lime, by M. J. Cavalier.—On the aluminium contained in mineral waters, by M. F. Parmentier. The author points out that in spite of numerous analyses of the waters from Puits Chomel and Grande Grille the presence of aluminium in notable quantity has been overlooked.—The action of isobutylene bromide on benzene in presence of aluminium chloride, by M. F. Bodroux. The principal products are a butyl-benzene and dimethyl-phenyl-benzyl-methane.—The action of the alkyl malonic esters upon the diazoic chlorides, by M. G. Favrel. Ethyl-methylmalonate, treated with a solution of diazobenzene chloride in presence of sodium acetate, gives the phenylhydrazone of ethyl pyruvate. Diazoparatoluene gives an analogous reaction.—On a new mode of decomposition of bisulphite derivatives, by MM. P. Freundler and L. Bunel. Alkaline nitrites may replace the alkaline carbonates in this reaction.—On the secondary products formed in the action of sulphuric acid upon wood charcoal, by M. A. Verneuil. The tetra-, penta- and hexa-carboxylic acids of benzene can be isolated from the residue.—On a new gregarian parasite of the mussel, by M. Louis Leger.—On the cilia of the Ctenophoræ and on ciliary insertions in general, by M. P. Vignon.—Experimental researches on the respiration of annelids. Study of *Spirographis Spallanzanii*, by M. Bounhiol.—The defensive or odorous glands of the cockroach, by M. K. Bordas.—On the structure of the shoot in ligneous plants, by M. Marcel Dubard.—On the proportion of water compared with the ripening of ligneous plants, by M. F. Kövessi.—On the electrolysis of animal tissues, by M. Édouard Branly.—The sources of iodine in the organism. The biological cycle of this metalloid, by M. P. Bourcet.—A method of preparing low brewery yeasts fermenting at a high temperature, by M. Georges Jacquemin.—The otoliths and audition, by M. Pierre Bonnier.—A case of trichosporia (*piedra nostras*) observed in France, by M. Paul Vuillemin.—On the thunderstorm in Paris of May 29, by M. J. Jaubert.

DIARY OF SOCIETIES.

THURSDAY, JUNE 13.

ROYAL SOCIETY, at 4.30.—Bakerian Lecture: Prof. James Dewar, F.R.S.—The Nadir of Temperature and Allied Problems. (1) Physical Properties of Liquid and Solid Hydrogen; (2) Separation of Free Hydrogen and other Gases from Air; (3) Electric Resistance Thermometry at the Boiling Point of Hydrogen; (4) Experiments on the Liquefaction of Helium at the Melting Point of Hydrogen; (5) Pyro-Electricity, Phosphorescence, &c.

MATHEMATICAL SOCIETY, at 5.30.—Remarks on the Quartic Curve $2\alpha^3\beta+m\beta^3\gamma+n\gamma^3\alpha=0$: A. B. Basset, F.R.S.—The Theory of Cauchy's Principal Values, II.: G. H. Hardy.—The Rational Solutions of the Equation $u^3+v^3+w^3+p^3=0$: Prof. Steggall.—Invariants of Curves on the same Surface, in the Neighbourhood of a Common Tangent Line: T. Stuart.

FRIDAY, JUNE 14.

ROYAL ASTRONOMICAL SOCIETY, at 5.—Observations of Mars made at Mr. Edward Crossley's Observatory, Bermerside, Halifax, during the Opposition of 1900–01: J. Gledhill.—A Modified Form of Revolving Occulter for Adapting the Exposure of the Sun's Corona to its Actinic Intensity at all Distances from the Moon's Limb: D. P. Todd.—The Oxford Determinations of Stellar Parallax—Reply to Prof. Turner: Sir D. Gill.—Sun-spots and Magnetic Disturbance: W. Ellis.—Observations of Nova Persei made at Birr Castle, Parsonstown: The Earl of Rosse.—Secular Variation in the Period of R. Carinæ: A. W. Roberts.—The Great Comet of 1901, as observed at the Royal Observatory, Cape of Good Hope: Sir D. Gill.—The Oxford Determinations of Stellar Parallax—Further Reply to Sir D. Gill: H. H. Turner.—Measures of Double Stars made at Mr. E. Crossley's Observatory, Bermerside, Halifax: J. Gledhill.—Corrections to reduce the Revised Madras Catalogue of Stars for 1835·0 to the Fundamental Catalogue of Auwers: A. M. W. Downing.—The Lyrids, 1901 April, observed at Cambridge: J. C. W. Herschel.

PHYSICAL SOCIETY, at 5.—On Herr Jahn's Measurements of the Electromotive Force of Concentration Cells: Dr. R. A. Lehfeldt.—Exhibition of a Set of Specimens of Jena Glass: Prof. S. P. Thompson, F.R.S.

MALACOLOGICAL SOCIETY, at 8.—Notes on *Ariophanta*, *Xestina*, *Nilgiria* and *Euplecta*: W. T. Blanford.—Pleistocene Shells hitherto unrecorded from the Raised Beach of Perim Island, Red Sea: Rev. R. Ashington Bullen.—On a Dibranchiate Cephalopod from the London Clay of Sheppy: G. C. Crick.—(1) Description of a New Species of *Acanthochites* from South Africa; (2) Description of a New Species of Helicina from the Pelew Island: E. R. Sykes.—On the Anatomy of *Helix politissima*, Pfeiffer, and its Generic Position in the Ariophantinæ: Lieut.-Colonel H. H. Godwin-Austen.

TUESDAY, JUNE 18.

ZOOLOGICAL SOCIETY, at 8.30.—Observations on some Mimetic Insects and Spiders from Borneo and Singapore: R. Shelford.—Further Researches upon the Molluscs of the Great African Lakes: J. E. S. Moore.—On the Collections of Birds made by Dr. Donaldson Smith in Northern Somali-land: Dr. R. Bowdler Sharpe.

MINERALOGICAL SOCIETY, at 8.—On the Anharmonic Ratio of Four Faces in a Zone: Alfred Harker.—On the Arrangement of the Chemical Atoms in Potassium-Alum and in some of the Bodies which display Tetartohedral Symmetry: William Barlow.—Remarks on Calaverite: Herbert Smith.

ROYAL STATISTICAL SOCIETY, at 5.—The Recent Gold Production of the World: Wynnard Hooper.

WEDNESDAY, JUNE 19.

GEOLOGICAL SOCIETY, at 8.—On Intrusive Tuff-like Igneous Rocks and Breccias in Ireland: J. R. Kilroe and Alexander McHenry.—The Use of a Geological Datum: Beeby Thompson.

ROYAL METEOROLOGICAL SOCIETY, at 4.30.—The Eclipse Cyclone, the Diurnal Cyclones, and the Cyclones and Anticyclones of Temperate Latitudes: H. Helm Clayton.—The Seismograph as a Sensitive Barometer: F. Napier Denison.

ROYAL MICROSCOPICAL SOCIETY, at 8.—Examination of the Abbe Diffraction Theory of the Microscope: J. W. Gordon.

THURSDAY, JUNE 20.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—On the Freshwater Algæ of Ceylon: W. West and G. S. West.—On Coprophilous Fungi: George Massee and E. Salmon.—Revision of the Genus Hypericophyllum, Steetz, with Notes on certain Genera with which it has heen confused: N. E. Brown.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—The Direct Union of Carbon and Hydrogen, Part II.: W. A. Bone and D. S. Jerdan.—Ammonium and other Imidosulphites: E. Divers and M. Ogawa.—Nitrilosulphates: E. Divers and T. Haga.—The Decomposition of Hydrocarbons at High Temperatures: W. A. Bone and D. S. Jerdan.—The Sugars from Cellulose: H. J. H. Fenton.—On a Theory of Chemical Combination: G. Martin.—On the Occurrence of Paraffins in the Leaf of Tobacco: Dr. T. E. Thorpe, C.B., F.R.S., and John Holmes.—Studies in the Camphane Series, Part IV.: M. O. Forster.—On the Decomposition of Carbon Dioxide, when submitted to Electric Discharge at Low Pressures: Dr. J. N. Collie, F.R.S.—Two New Substances in Lemon Oil: H. E. Burgess.

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